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AN INTERFERENCE THEORY OF COLOR VISION

By W. T. M. FORBES, Cornell University

At present two principal theories of color vision hold the field. The so-called three-color theory, with which the names of Young and Helmholtz[1] are associated, appeals specially to the physicists, but the psychological difficulties seem hopeless, especially in the interpretation of violet and brown. It is interesting that some of the physicists who have worked on it most professionally confuse blue and violet so persistently in their writings as to raise the suspicion that they may be violet blind.[2] The alternative theory is usually associated with the name of Hering,[3] but his formulation appears needlessly complex to the physicists and seems to assume physiological processes for which we have no precedent. Ladd-Franklin[4] has developed a scheme which avoids most of the difficulties of the other two, but which requires that each retinal cell shall send to the brain at least three separate types of stimulation. As there is only a single fibre to each cone or rod, these three (or more) excitations would have to be carried in a single fibre, which would be unprecedented, as in all the cases yet examined a single fibre appears able to produce only a single kind of reaction, with variation in frequency alone.[5]

As is well known, it is possible to make an array of all conceivable colors in a figure of three dimensions (double cone or prism), and not less—so in the interest of simplicity it seems best to assume that there are also three processes involved in the transmission of each colored stimulus to the brain, each one variable only quantitatively. Both the familiar *three-color* and *four-color* theories in fact have this property, the three independent variables in the four-color scheme being represented by red-green, blue-

*Accepted for publication January 16, 1927.

[1] H. von Helmholtz, *Treatise on Physiological Optics*, American ed., II, 141 ff.

[2] In the sense merely of inability to distinguish violet from blue as hues.

[3] Summarized with full references in J. H. Parsons, *An Introduction to the Study of Colour Vision*, 251-263.

[4] Helmholtz, *op. cit.*, II, 455-468; Parsons, *op. cit.*, 271-274.

[5] E. D. Adrian, *J. Physiol.*, 61, 1926, 65, 465; 62, 1926, 33.

yellow and white-gray-black; in the three-color by white-green-black, white-red-black and white-blue-black, or a similar trio. It has likewise been noted that the three variables: dominant wave-length, intensity, and saturation, would also serve, but no color-theory is built on this trio, and it is difficult to imagine an end-organ for the detection of saturation independent of hue or intensity. This method of analysis, however, has proved very useful in the practical specification of color.

To prevent the difficulty of having a single nerve carry more than one process, it will be necessary to assume three fibres involved in each colored sensation, and therefore three cones (or rods).

In the hypothesis here presented I endeavor, using three kinds of cones, to avoid the difficulties of each of these theories, without assuming the existence of substances, structures, or processes which would be unprecedented in living tissues. I do not consider anything that happens behind the retina, and so will not fully treat the phenomena of fatigue and contrast, or many others which seems to involve the function of ganglia as well as the visual cells. I also will give little attention to the so-called rod-vision. In its typical form it is wholly uncolored, and except in the matter of certain after-images it can be viewed as merely a form of white-vision associated with an extreme degree of light-sensitiveness. It is very questionable if the current accounts are correct; in fact there is no clear evidence that the so-called rod-vision and cone-vision are in fact limited to the rods and cones. The whole matter should be worked over from the point of view of the gradually varying structures of different zones of the retina. At present the clear accounts of retinal structure apply mainly or wholly to the peripheral parts, while the physiological data which have been applied to them are mainly or wholly derived from central and paracentral regions of the retina with a far different structure. Incidentally the end-organs in the fovea are not slightly modified cones, as most text-books state, but approximate much more closely in appearance and, according to Cajal, in their nerve-connections also, to the rods. In the remainder of this paper I shall use the term 'cones' indifferently to indicate visual end-organs, with the proviso that they may in fact turn out to be in part the rods in the peripheral retina, and certainly in the central retina include the various modified and more or less rod-like structures that exist there. I have a private suspicion that in the peripheral retina, where true cones exist, they are the color-organs only, and that the rods provide the white element at photopic as well as scotopic levels, there being two pigments rather than two structures involved.

The hypothesis presented in this paper requires:

(1) *A structure of dimensions of the order of the half wave-length of light,* and of moderate uniformity. A familiar example of such a structure is the thin reflecting film which produces the iridescence colors of birds and

butterflies. Certain structures in the muscle-striations are examples in soft tissues of comparable fineness and sufficient uniformity.

(2) *A light-sensitive substance*, with its color-sensitivity proportional to the known sensitivity of the retina. The familiar visual purple is a substance of the proper type, but both from its absorption curve, and its easy bleaching with moderate amounts of light, it has been without question associated with the sensitiveness to light of low intensity. For photopic vision one would have to postulate a similar pigment with its maximum absorption slightly nearer the red end of the spectrum. As such a substance would be bluer than the visual purple, I shall refer to it as a visual violet. The ordinary method of preparing the visual purple under red light would not reveal it, as it would be bleached by red light,[6] and in any case it need be present only in such minute amount as possibly to be invisible, like the dyes in an ortho-chromatic plate; but there has actually been revealed some slight evidence of a visual violet, momentarily laid bare by rapid bleaching of a dark-adapted eye.

(3) *A fine membrane* which shall either be an insulator of moderate efficiency or semipermeable. The ordinary cell and nuclear membranes are structures of this character. It is possible that this structure could be dispensed with, the nerve-excitation being caused by a gradient within the cell which would produce an electric or chemical polarity, but the statement of the theory is simplified by assuming a membrane, and it is by no means an improbable structure.

(4) *Three kinds of 'cones'* in the color-sensitive parts of the retina. There should be differences of structure which might *possibly* be visible, but which would be at the limit of visibility.

(5) *Post-retinal structures* capable of explaining the effects of contrast and of the reflex part of adaptation, and perhaps of the black sensation so far as it seems even darker than the idioretinal level. The 'granular' layers of the retina and more central ganglia provide such structures in abundance.

(6) *A restoring mechanism* tending to bring the cell back to a resting state. The imperfect insulation provided by any organic structure would discharge any electrical accumulation. A restoration of the light-sensitive substance is needed for any theory, and is well represented by the well-known recovery of the visual purple (in the case of photopic vision represented by the postulated visual violet) when unstimulated.

(7) *Explanation of after-images.* The two restoring processes just mentioned if somewhat sluggish (as they undoubtedly are) would explain positive and negative after-images, respectively. Some slight analysis is made of this phase in the appendix. There is also perhaps some slight contribution by central processes of persistence and of fatigue.

The Interference Theory

(A) *The receptive substance.* For this I assume that there exists a pair of visual pigments, one the familiar 'visual purple'[7] of scotopic vision, the other a closely similar substance with a

[6] E. Köttgen and G. Abelsdorff, *Zsch. f. Psych. Phys. d. Sinn.* 12, 1896, 161; A. König (with Köttgen), *Sitz. k. Pr. Akad. d. Wiss. zu Berlin*, 1894, 577 (collected papers, 338). König and Kühne worked with orange sodium light, Köttgen and Abelsdorff with deep red light. Their respective curves tend to show irregularities at about 580 and 660 mμ respectively, suggesting both that the light may not have been without selective influence, and that there may be more than one photopic pigment involved.

[7] This term is in fact a misnomer, the visual pigment of scotopic vision being in fact a rose and not a purple, but it is too late to make the change now, the term 'visual purple' being so fully established in the literature. Similarly the other pigment which I assume, should really be called a purple, as it would transmit both ends of the spectrum more or less equally; but the *relation* in absorption will be sufficiently indicated by the term 'visual violet.'

curve of sensitiveness corresponding to the apparent brightness of the spectral colors to the light-adapted eye. This second substance would have a maximum of absorption in the yellow rather than the green, and I will call it therefore the 'visual violet.' It should exist in the eye only in a minute and probably invisible amount, in fact in about the same proportion to the visual purple as the ratio of maximum photopic and scotopic sensitivities—some such matter as 1:1000.

It is usually assumed that these two pigments are associated with distinct end-organs. There seems to be no necessity of this,[8] and the tendency of colored stimuli to change their hue near the threshold of color-sensivity seems to indicate that the color-cones also possess the scotopic as well as the photopic pigment, though in far less quantity than the more peripheral of the white-organs (rods). Only in the fovea is there no definite evidence of two pigments.

As a tentative suggestion I will assume that the visual pigment when acted on by light is oxidized, producing acid[9] accumulation at the point stimulated. If this is the portion of the cell to which the nerve is attached, the activity of the nerve is increased and a sensation produced. This acid will gradually dissipate, but it is a physical substance independent of the light-stimulus itself, and until it is discharged it will continue to stimulate the nerve, thereby causing a positive after-image of gradually decreasing intensity.

I assume that the visual pigment exists in a thin layer (at least in the color-cones) near the tips of the cones.

I assume further that the oxidized visual pigment is not itself sensitive to light, with the result that until it is again reduced by some external substance (not inconceivably the sugar of the lymph) there is a decrease of light-sensitivity of the cone or layer of the cone affected, and if this does not take place till after the 'acid' is dissipated and the nerve no longer stimulated, there will result a period of negative after-image.

The existence of the idioretinal light would indicate that there is some spontaneous decomposition even without the presence of light. To explain the existence of negative after-images in complete darkness it would have to be assumed that the oxidized pigment is incapable of this decomposition as well as that due to light. This is to be expected, as chemically the two processes are no doubt alike.

It will be seen that the chemical mechanism postulated is that usually assumed for this as well as many other physiological processes, the oxidized pigment being a 'precursor' and the reducing substance being the activator. The visual pigment should not be viewed as being destroyed, but as alternating between a light-sensitive and a chemically active state.

(B) *Receptive structures.* It will be necessary to assume three types of cones (see Fig. 1).

(1) *White cones* (perhaps rods in the outer part of the retina). The white cones do not need to have any particular structure nor location of the visual pigment. It will be assumed

[8] This point is discussed more fully on p. 21. Cf. Fig. 5.

[9] It is known that the retina becomes acid on stimulation. For reference see Parsons, *op. cit.*, 12.

that the whole pigmented portion of the cone (in the rod the whole rod) is in touch with the nerve-ending and serves to stimulate it, and that there is either no inhibitory effect as suggested below for the color cones, or else that it acts only between neighboring cones (contrast). The automatic activity of these cones is assumed to be slight and to cause a dark gray idioretinal light—the most inconspicuous color imaginable. The more positive sensation of *black* is assumed to be the result of a central process—inhibition by the activity of neighboring neurones[10] or less probably to a reflex cutting off of the restoring substance. The waves of relative blackness and brightness which pass over the retina in the dark at certain levels of dark-adaptation are more probably due to (vasomotor) waves alternately cutting off and supplying extra amounts of restoring substance.

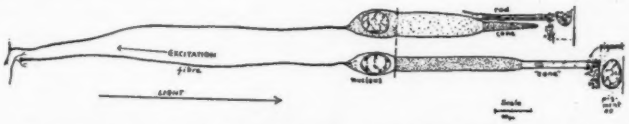

FIG. 1. Above: typical cone cell from peripheral part of retina, and apical portion of a rod. Below: typical foveal organ, intermediate between rod and cone. The figure is semi-diagrammatic, mainly based on the retina of a monkey in the collection of the Cornell University Medical College.

Activity of these cones will by itself produce the series of grays and white, and the range of sensitivity must be assumed to be greater than in the case of the color cones, explaining the persistence of the white sensation when the light is too faint to stimulate the sense of color; and also continuing to increase with increase of intensity long after the color cones have reached their maximum, and in the end wholly covering and obliterating the colored sensations.

The first of these effects would naturally result from the existence of a greater quantity of visual pigment just as well as from a greater sensitivity; the second, I assume, would be a result of loss of the color-sense by effectively complete bleaching of the pigment in the color organs while enough of the larger amount still survives in the white-organs to function.

(2) *Yellow cones* (yellow-blue). In the case of the color cones I assume the existence of a characteristic structure. The apex of a cone is turned away from the source of light (see Figs. 1 and 2) and ends bluntly, being immersed in a probably less refractive lymph or undifferentiated substance (the cone being highly refractive). This will produce a certain amount of reflection of light, which will interfere with the arriving wave and

[10]There is evidence from after-images that this inhibition is exercised on color as well as white sensations.

produce a series of interference maxima and minima in the substance of the cone. I assume that the visual pigment exists in a thin layer at the apex of the cone, so thin that only the first (or in the case of violet light the second also) of the maxima falls within it.

At each of these maxima there will be a maximum of photochemical activity, exactly as in the case of the Lippmann process of photography, where there is a similar reflection, and silver plates are laid down at the interference maxima. In the case of homogeneous colored light these lines of maximum activity will be sharp, while in the case of ordinary mixed lights, there will be a band of activity corresponding to the whole

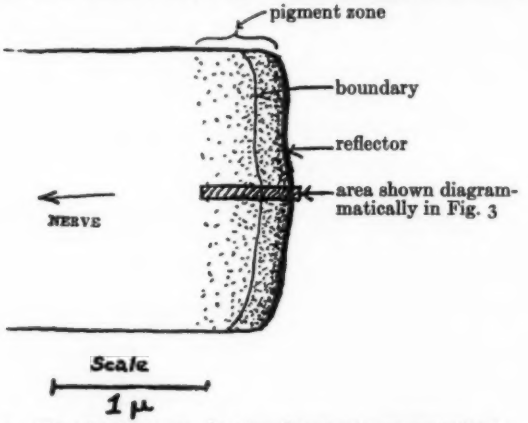

FIG. 2. Tip of color-cone, showing the structures postulated.

assortment of wave-lengths present; but in any colored light there will be a maximum corresponding to the dominant wave-length. Now we can imagine the existence of a semipermeable film, similar to a nuclear membrane in character, parallel to the reflecting end of the cone and at a distance from it equal to half the wave-length of the primary green light (say 500 mμ).[11] In this case yellow light would produce a maximum which would lie above the film, that is on the side of it which was connected with the cell-body and its efferent nerve, while blue light would similarly cause activity on the further side of it (toward the choroid), and would be insulated from the cell-

[11] There is a tendency, perhaps more prevalent among psychologists than physicists, to use the symbol μμ to represent the micromillimeter (.000,000,001 M.). As m everywhere in the metric system stands for one thousandth, and μ for one millionth, mμ is the normal combination for a thousand-millionth. Incidentally, with the recent work on radioactivity, μμ is coming into use in its proper meaning of one millionth of a millionth (.000,000,000,001 M)

body. There should also, in equilibrium, be equal quantities of the visual pigment above and below this film or boundary.

(3) *Red cones* (red-green). The red cones may be considered as of exactly the same structure as the yellow ones, save that the distance from the back surface of the cone to the film is a little greater, corresponding to half the wave-length of yellow light (say 575 mμ), and the visual pigment is correspondingly deeper, so that it is still halved by the film (in quantity) and some of it reaches so far from the reflecting surface that in the case of the shorter waves (blue and violet) the second interference-maximum as well as the first will fall within the band of visual pigment, the two bands falling on opposite sides of the boundary film.

The result, then, in the case of the short waves will be to produce the same effect as the longest (that is the red sensation). In the blue these effects will be assumed to be equal in amount, and neutralize each other, while in the violet the second maximum will fall in a region of greater sensitiveness than the first, so that the net effect will be to excite the red rather than the green sensation.

It is to be noted that on the short-wave side of the boundary in each cone there is a narrower sensitive band than on the long-wave side. We assume, therefore, considering that white light produces no net effect in the color cones, that there is a higher *concentration* of pigment on the short-wave side (toward the choroid) gradually fading out toward the cell-body. This is entirely probable, as the phenomena when the retina is separated from the choroid indicate that the restoring substance for the visual pigment is supplied from that side. Not only does a retina separated from its choroid fail to recover its visual purple,[12] but it is interesting that the yellow-blue organs whose sensitive layers would, according to this theory, be both thinner and nearer the back of the retina, are in fact the first to be destroyed in cases of separation of the retina.[13]

This also explains a curious, and I believe hitherto unexplained fact, viz., the effectiveness of light of different colors in adding to the intensity of white light, for instance as measured in a flicker photometer, does not correspond at all to its intensity as measured by its power to neutralize a complementary light. As we go progressively toward the violet end of the spectrum the light rapidly becomes more effective in its neutralizing (color) power. This would naturally follow from such a gradient of visual pigment as the other evidence suggests, for the white value would correspond to the *chemical* effectiveness of the light in acting on the visual pigment in the white cones, while in the color cones its effectiveness would depend on the product of its chemical effectiveness with the concentration of pigment at the level where the band of activity lies. As the pigment becomes more concentrated toward the reflecting surface (where the shorter waves fall), the effectiveness of the light (for color only) would increase in proportion. The second maximum at the red end of the red-green cones would also be doubled in color-effectiveness, because of the spreading out of the second order spectrum, which will be twice as long as the first.

[12] Parsons, *op. cit.*, 14.
[13] Parsons, *op. cit.*, 180; A. König, *Sitz. k. Pr. Akad. d. Wiss. zu Berlin*, 1897, 720. Note especially König's report on observers 7, 8, and 9.

If for any reason the band of visual pigment in the red cones were less deep than usual, the result would be that the observer would not see a red component in violet light at all, but would tend to confuse violet and blue, which would differ in saturation only, if at all. I believe I have come across one such person (DBJ), who, so far as we know, is otherwise normal. He chooses as his best blue a point in the spectrum about 450 mμ, where the normal person chooses 475 or 480 mμ, and sees no suggestion of redness at the violet end of the spectrum. He is also of somewhat subnormal sensitiveness at the red end of the spectrum, apparently seeing the extreme reds a little darker than the normal person. This indicates that not merely the portion of the visual pigment corresponding to the second maximum of the violet rays (about $2 \times 450 = 900$ mμ) is absent or so weak as to be wholly dominated by the green-activity of the first maximum, but that there is also an abnormally small amount as far down as 700 mμ or a little farther. This would be practically the distribution which would be normal for the yellow cones, and it is a plausible suggestion that in this case one of the differentiating factors between the two types of cones has been lost. There is evidence that in the case of the deuteranope the other differential only is lost, the light-sensitiveness corresponding to the normal range for the red cones, though there is no distinctive red-sense. How common is this type of defect? A good many cases in the literature could be cited where 450 mμ is chosen as a representive of blue, and even where the same writer uses the terms blue and violet as equivalent. Perhaps some of these observers are also violet-blind, though in some it may be merely a careless use of color words. Still I think the normal observer would be far more likely to confuse violet with purple than with blue.

The substantial equality of the blue-yellow and the red-green sensations suggests that the two kinds of organs are probably present in the retina in equal numbers;[14] the white cones are no doubt present in larger (perhaps much larger) numbers. It would be interesting to determine if any such pair-wise arrangement of the cones could be detected in the actual structure of the retina. The retina is made up, as usually described, of little circular groups of cones and rods (a ring of rods about a central cone in the peripheral retina, much like the so-called retinulae of the insect eye). It would be worth while to hunt for a zone in which the cones were arranged in pairs.

(C) *Process.* Light falling on any point in a cone causes activity (and exhaustion) of visual pigment in proportion to its intensity at that point, and to its sensitivity to the wave-length concerned.

In the white cone all activity causes a like effect and a sensation in proportion merely to the amount of light of each wave-length $\times$ the sensitivity (integrated over the various wave-lengths present) $\times$ the amount of visual pigment in an active state at that moment.

In a colored cone we may divide the light into two portions (see Fig. 3). First, there will be the light that passes through the cone and out, without reflection, and therefore without producing maxima and minima. This will cause an equal effect on all parts of the cone exposed to it, and on the assumption

[14]The evidence for equality of the two processes is not too tangible, and should perhaps not be emphasized. It is mainly that colors which may be considered equally saturated make about an equally strong impression on the consciousness, allowing for the interference of the white sensation in the middle of the spectrum, and for the processes of inhibition (blue and green) being weaker than those of direct stimulation (red and yellow).

that the amount of pigment is equal on the two sides of the boundary film, will leave the cone as a whole in a balanced and inactive condition. White light also will not produce any bands, and will therefore produce an equal action on both sides and no net result, except an exhaustion of the visual pigment and thereby a weakening of the color process.

If the light is colored, however, there will be a more or less definite maximum produced by the part of the light which is reflected, at a level corresponding to its dominant wavelength. This will cause more active material to accumulate at this level, and produce a chemically polarized condition of the cone. I assume (for the purpose of a formulation) that the active layer on one side of the membrane and the inactive layer on the opposite side act as a sort of battery and cause an electrical accumulation on the side which is active.[15] This, if it is on the side of the boundary to which the nerve is attached stimulates the nerve and causes a discharge more rapid than the spontaneous rate. If it is on the opposite side. the current will be away from the part attached to the nerve, and will cause a corresponding degree of inhibition.[16]

If the active band lies astride the boundary, the two sides will work against each other and the effect will be weakened, till in the case where it is evenly astride there will be no net activity and no sensation.

I assume that the cone when in an unstimulated state has a characteristic rate of discharge, which like many other nerve-conditions of tonus does not stimulate the central nervous system, allowing the latter to produce the sensation of mid-gray with which it seems to fill all areas (like the blind spot, for instance) which are wholly unstimulated.

I assume, furthermore, that of the four primary colors, red and yellow are produced by overactivity of their respective organs, and green and blue by inhibition. Yellow and red, then, might perhaps be thought of as successive differentiations of white, and blue and green similarly of black.

(D) *Revival.* I have assumed that within the range of ordinary color sensitiveness the visual pigment is restored at a constant rate. Undoubtedly this is not strictly true, as the rate of restoration is no doubt dependent in part on the supply of the restoring substance, which, in case it is derived from the circulating blood or lymph, may be under reflex control; and there may also be other disturbing factors. Nevertheless in a

[15] An equally plausible assumption is that the colloidal condition of the protoplasm is changed, and causes a drift of water in one direction or the other, the resulting dilution or concentration being the immediate stimulus.

[16] There is some evidence from a consideration of minimum distinguishable steps, that fewer distinguishable degrees of inhibition are possible than of activity; and Steindler's data, when plotted in a curve, seem to point in the same direction (Fig. 4).

broad way the statement is true. The result is that in a steady state of illumination the pigment must be bleached at the rate it is restored; for if it were bleached more rapidly, there would be a decrease in its total amount, and a corresponding decrease in the activity, for the number of molecules decomposed in a given time in a given intensity of light is no doubt proportional to the number present. Similarly less rapid bleaching would promptly allow an accumulation of pigment, and an accelerated rate. As a result of this, in the white cones there would tend to be a constant activity of the nerve under a steady illumination, regardless of the intensity. In fact there is a little variation, but it appears that a change of a good many thousandfold in the brightness of illumination only causes a very moderate change in the sensation, which remains well within the range of a mid-gray. There is evidently, however, a slowing up of the accumulation of pigment as the cell approaches saturation, and no doubt eventually a complete cessation, and similarly the accumulation seems sluggish in a completely exhausted cone,[17] which suggests that the process may follow something like an autocatalytic curve.

In the color cones a different condition arises. The visual pigment is evidently very sluggish in diffusion, perhaps even non-diffusible, while the restoring substance, being supplied from outside, must be easily diffused. Therefore in this case the inequality in distribution of the purple will remain so long as the unbalanced light is applied, and the equilibrium of rate of decomposition to rate of restoration will only apply to the cone as a whole. Color then will tend not to be completely fatiguable, but merely will be decreased; for the white light, and the light that fails of reflection, will exhaust the purple in the whole cone, and lessen the possible degree of polarization. There should exist with brilliant illumination, then, a paling of all colors proportionately, substantially what the artist represents, when painting them in a high key.

(E) *Explanation of particular colors.* Here follows an analysis of the effect that light of certain particular colors might be expected to have on the three kinds of cones (see Table I and Fig. 3).

Red (Fig. 3, lines 3 and 4). I have assumed that the stimulation of the white cone, which must exist, is negligible with moderate illumination. The red cone will have its entire band of stimulation in the near (red) side and of full intensity. The yellow cone is also stimulated, and of considerable intensity, thereby explaining why adding some blue light to a spectral red produces a more primary hue.

I think most people choose a carmine red as the pure color, unless their judgment has been changed by familiarity with the spectrum, rather than the spectral red. The carmine red is also the color which remains unchanged

[17] By completely exhausted, I mean exhaustion to the point of no longer being able to initiate an impulse in the nerve-fibre. Whether all trace of pigment is exhausted, or whether the amount indefinitely approaches zero as the light is increased is a matter of secondary importance.

as one goes toward the periphery of the retina, while the spectral red changes to yellow when one passes outside the zone of red-green sensitiveness. I judge that the amount of blue which is mixed with spectral red to produce this result is the amount which equals the yellow activity caused by the red light striking on the yellow cone.

Pink. Cited as an example of a color lighter than the spectral color. The white admixture increases the activity of the white cones, but in color cones merely exhausts the visual pigment, and thereby reduces the intensity.

Many people see the pink produced as a dilution of spectral red without any conscious element of yellowness (I happen to be one). I believe that in such cases the yellow activity—which is undoubtedly less than the red activity under red light, the band falling farther from the boundary film and nearer the level where the visual pigment fades out—falls below threshold level, and the red and white sensations only are stimulated.

TABLE I

Color	White Cone	Yellow Cone	Red Cone
Red	(+)	+	+ +
Orange	+	+ +	+
Yellow	+ +	+ +	o
Green	+	o	—
Blue	+	− −	o*
Violet	(+)	− −	+†
Purple (red + blue)	(+)	−	+ +‡
White	+ + +	any	any
Gray	+	o	o
Black	o	o	o
Brown (yellow − low intensity)	(+)	+	(+)
Pink (red + white)	+ +	(+)	+ +

o = No effect, or no net effect.
+ = Moderate intensity.
+ + = Strong intensity.
+ + + = Maximum intensity, reached by white cone only.
(+) = Theoretically functioning, negligible in proportion to other cones.
* First order in the negative and second order in the positive of red cone equally strong.
† Second order.
‡ Both orders, the first order of the red and second order of the blue light.

Yellow (Fig. 3, lines 7, 8). Yellow light is the color of maximum effect on the visual pigment in the white organs and stimulates them to the point of overshadowing the color process. The yellow cone is of course at maximum activity, while the band in the red cone falls astride the boundary film and is neutral in effect. We then have a maximum yellow process combined with a very high white-process.

Decreasing somewhat the intensity of the light, or what is much the same thing, lowering the white-sensitivity by adapting the eye to a brighter surrounding field, will to a certain point seem to increase the saturation of the color. Perhaps the true primary yellow should be chosen a deeper shade than is usually done (the spectral yellow as seen against a dark background), but it is questionable whether brown should be substituted for yellow.

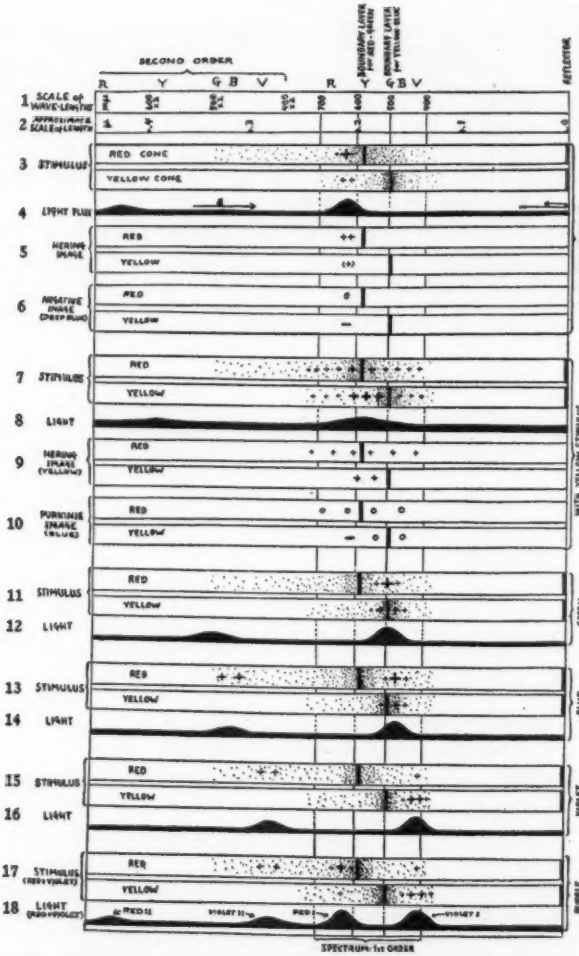

FIG. 3. Illustrating the formation of standing light-waves in the two kinds of color-cones, and the relations of reflecting apex of the cone, boundary film, and layer of sensitive substance.

The upper line (line 1) indicates the positions at which the standing waves would fall for the various wave-lengths. These are at distances back of the reflecting surface equal to half the wave-length in air divided by the index of refraction of the cone-substance, which for the purposes

Brown. A typical brown will have the same maxima in the color scheme as yellow,[18] but the intensity has fallen to a point where the white sensation no longer dominates the picture. On the other hand the yellow process itself is certainly not functioning at its maximum intensity, and brown hardly represents the true primary color, which should have the saturation of yellow, but more nearly the luminosity of brown.[19] The primary might be described as a yellow *minus* white.

Blue (Fig. 3, lines 13, 14). In the red-green cone it is assumed that both the first and second interference maxima are functioning, and that they are of near enough equal strength and symmetry to neutralize each other, the first lying on the green side of the insulating film and the second on the red, further out than the red maximum would fall. In the blue-

[18]Or orange, but I take yellow as a type to analyse.

[19]The same is true to a less degree of all the colors, as all can be stimulated only along with the white sensation to a greater or less degree, but the effect reaches a maximum in the middle of the spectrum. One would expect it to be equally strong in the yellow-green, but by the time we reach the invariable (presumably primary) green of around 500μ it has fallen off considerably, and we may assume we experience something like the true primary green sensation.

of a diagram I have taken as 1.5. The second line (line 2) shows the actual scale of measurement in microns.

In the other lines (lines 3-18) the two oblongs are diagrammatic strips from the apices of the two kinds of color-cones, corresponding to the shaded area in Fig. 2. The reflecting surface and boundary film are indicated by heavy bars, the sensitive band by stippling of a density to correspond with its *effective* sensitiveness (the combined effect of the density of substance and its sensitivity to the wave-length that falls at a given point).

The black band labelled 'light flux' diagrammatically shows the light passing in from the base of the cone, reflected in part from its apex, and producing standing waves represented by the widenings of the band, which form the effective stimulus. These bands should be imagined as superposed on the cones to which they are bracketed. Finally the characters in the cone-diagrams (+, o) indicate the strength and position of actual stimulus—the product of sensitiveness of the cone (stippling) by amount of light (black stripe).

Hering image (lines 5, 9). The small plus signs indicate the residual positive activity a moment after the actual stimulus and its resultant activity (lines 3, 7) ceases. It is like the reaction to the continuing stimulus, but weaker.

Purkinje image (line 10). The stimulus (line 7) has fallen on a saturated cone (therefore not actively regenerating the visual pigment) and is of intensity too low to accumulate active substance and leave a surviving Hering after-image. There is therefore no activity surviving, but a defect due to fatigue at the point of maximum previous activity in the yellow cone (indicated by a *minus* sign) which throws that cone out of balance and leaves a blue (*minus* yellow) sensation. With red light (line 6) there is no Purkinje image, but after a strong stimulus and its resulting Hering image the less strongly stimulated yellow cone goes into its negative condition more rapidly than the strongly stimulated red one, resulting in a blue negative condition.

The diagram for purple (lines 17, 18) is merely a superposition of those for red (3, 4) and for blue or violet (15, 16).

yellow cone the first maximum falls on the blue side, while the second falls beyond the end of the pigment area and is inactive.

Violet (Fig. 3, lines 15, 16). Here the second maximum is assumed actually to overpower the first, so that the red-green cone is functioning actually as red, the stimulation being greater than the inhibition. Note that the *wave-length* of the light serves merely to determine its effect on the visual pigment while the *position* (in this case dependent not on the wave-length itself but the *double* wave-length) determines the color reported.

Purple (Fig. 3, lines 17, 18). To obtain a real purple we have to combine red with blue or violet light, omitting the intervening colors. In my diagram of this color I have assumed a mixture in which the violet band is enough stronger than the red to have a dominant effect on the yellow cone, while on the red-green cone the red band is fully functional and the violet (or blue) produces a second-order band to strengthen it further.

It is plain that a larger proportion of blue than that necessary to produce purple will produce a violet (only of less than spectral saturation), the red partially neutralizing the effect of the blue on the yellow cone, while in the red cone the red is fully present, but the blue serves only to exhaust the cone. Similarly with a greater excess of red, the small blue band will merely cancel the effect of the red light on the yellow cone while falling outside the region of strong activity in the red cone, so that a pure red activity will be left (but of less than spectral saturation).

Spectral red plus green. According to Ladd-Franklin spectral red plus green produces yellow. In the proposed theory the white cone will function moderately (average of red and green effects), the yellow cone moderately (the green falling on the neutral part and the moderate yellow stimulus caused by the red surviving), and the red-green cone not at all, the red and green neutralizing each other. In sum there will be the same combination as in the case of yellow light, but with each process of lower intensity. Of course as noted by Hering,[20] a red and a green can be chosen that will neutralize to produce white, and Hess[21] finds that as closely as can be measured these are the exact colors which drop out together without exposing any yellow or blue under-tint in the peripheral retina. No doubt they are the true primary colors.

Blue plus yellow. According to all authorities blue plus yellow produces white. The yellow will stimulate the white cone strongly, the yellow cone will have strong but evenly balanced stimuli, resulting in no excitation, and in the red cone the yellow will fall at the zero position astride the boundary film,

[20]Parsons, *op. cit.*, 68.
[21]C. Hess, *Arch. f. Ophth.*, 35, 1859, 1. See Parsons (*op. cit.*, 70) for further references.

while for the blue the first and second interference maxima will be evenly balanced. Therefore there will be no color sensation, but a strong white.

Discussion

In going over published work to find data which tend either to confirm or contradict this theory, a curious fact stands out very strongly: practically all the experimental work has been done at a level of adaptation where the eye is most unstable and sensitive to changes of illumination, and where the phenomena are most complex. Furthermore a very large proportion of the work has been done with the parafoveal part of the retina (1°–6° circles), a region of whose distinctive physical structure practically nothing is known. As a result published data are very difficult to interpret on the basis of any theory. A few examples may be cited.

Abney,[22] attempting to determine the threshold sensitiveness of the retina allowed 12 min. of dark adaptation. It is now known from Piper's work[23] that adaptation goes on for an hour or more (and to a certain extent for many hours); and also that 12 min. after entering the dark-room is a period when adaptation is going on at a very high rate—for many observers at the maximum rate. This fact has been verified very strikingly by Judd.[24] The only probable explanation of Abney's result appears to be that there was a very substantial amount of stray light, which held the observers' retinas to approximately the level which a normal eye reaches at the end of 12 min. This naturally throws doubt on all the quantitative results he publishes, as there is no guarantee that this light was constant in the course of his experiments, and as at that level the eye responds very rapidly to changes of illumination. In other cases (as with Priest, in his work on the determination of white)[25] the eye apparently came into equilibrium with the average effect of an intermittent stimulus. In this case the eye level was fixed in the sense that the experiment could undoubtedly be duplicated, but it is equally certain that the eye was being tested at the point where photopic and scotopic phenomena were mixed, and it is very questionable if the results would apply to normal day vision, and not certain that even a moderate change in the brightness of the stimulus might not cause a large difference in the value.

Steindler[26] has shown the very striking differences that exist among normal individuals in relative color-sensitiveness, and it is to be presumed that her variations in the regions of maximum power of discrimination may correspond with differences in the wave-length which excites the four primary (invariant) retinal sensations, but so far as I know no one has made these two kinds of measurements on the same individuals. She also has not distinguished change of saturation from change of hue, nor reported either the brilliancy of the light nor the retinal state, further than to indicate that the light was not constant for different parts of the spectrum The retina appears to have been in equilibrium with the stimulus light, but it seems not to be clearly stated, and as the stimulus light was small and apparently surrounded by a black field it is perhaps questionable.

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[22] W. de W. Abney, *Phil. Trans. R. S.*, 190 A, 155, etc.

[23] H. Piper, *Zsch. f. Psych u. Physiol. d. Sinnesorgane*, 31, 1903, 161.

[24] D. B. Judd, this Journal, 38, 1927, 507-533. Cf. Fig. 9.

[25] I. G. Priest, *U. S. Bureau of Standards, Tech. Paper* 417. I have not seen the original report; in the summary in the *J. Opt. Soc. Amer.*, 5, 1921, 205 ff; there appears to be a contradiction in the statement of the condition of adaptation. his condition 2c being impossible.

[26] O. Steindler, *Sitz. d. Weiner Akad.*, 115, 1906, 115.

Rods and cones.[27] There is extraordinary disagreement as to the structure of the retina. The fact appears to be that typical rods and cones, as described in the text-books, only exist in the peripheral retina, and may be ignored in interpreting most experimental work. The fovea is said to contain only cones, but the more careful writers always make this statement with some proviso. The fact appears to be that the foveal organs are almost typical rods, and a monkey retina in the Cornell University collection agrees with this.

The distinctive characters of a rod appear to be: (1) it extends clear to the pigment membrane; (2) it is slender and nearly cylindrical; (3) its nucleus lies close against the limiting membrane; (4) its efferent fibre ends not in an arborization but in a knob, which is received in a basket-like dendrite, apparently connected with a single ganglionic cell; and (5) it contains visual purple.

In the first four of these characters the foveal organs would be *rods*. If we assume that the form and the pigmentation of the organs are not necessarily bound together, it would appear that the low sensitiveness of the foveal organs is correlated merely with the absence of visual purple. The second and fourth structural features and perhaps the first seem really adapted for accurate vision, and would actually imply that we may have to reverse the rôle of rods and cones. It seems more plausible that the cones of the outer retina are the color organs, and that the rods produce the white sense at all levels, and also the finer details, for which their one-to-one connection with the ganglion cells would seem to be adapted. In that case the diffuseness of nocturnal vision would have to be explained as due to the withdrawal of pigment, and probably also to the coming into function of tangential connections in the deeper layers of the retina. The third after-image (diffuse) would be also explained in the same way, and its tardiness and diffuseness is quite what would be expected of cells of that type.

A good many experiments on foveal vision are vitiated by the use of too large a stimulus. The fovea is only about ¾° in diam.[28] and various experiments reported as foveal use stimuli of a diameter as large as 2°. This complicates the analysis of reports on supposed foveal after-images. In fact, with my own eye the fovea seems to have a bias toward a flash-image of red-yellow[29]—it is apparently thrown into greater activity by all colored stimuli. This is difficult of interpretation, and equally difficult with all theories thus far proposed.

Visual violet. Visual violet is an unsatisfactory term; the pigment should in fact be a purple, but the term visual purple is already preëmpted for what is in fact a rose. The fishes[30] appear to have a pigment much more like what I should like to postulate, the maximum absorption being at 540 mμ, fairly in the center of the yellow, about where the maximum bright-

[27] J. H. Parsons, *An Introduction to the Study of Colour Vision*, 203 f. In what follows I shall refer frequently to this book, which gives summaries of the work on the various fields, and full references to original reports of experimental work. I have gone back to the sources in many cases to find if conditions were not more strictly defined than would appear from Parson's account, but usually have been disappointed.

[28] Parsons, *op. cit.*, 9, gives it about .3 x .2 mm.

[29] Flash-image yellow with yellowish or blue-green stimulus alike.

[30] Köttgen and Abelsdorff, *op. cit.*, 171 ff.; Parsons, *op. cit.*, 13.

ness is in man in the lower stages of color-adaptation (mesopic).[31] It is possible that this is due either to a more primitive substance which is differentiated into two substances in man, or to an equal mixture of the two substances—the postulated visual violet being in larger quantity in the fish.

Purple and violet. There is considerable confusion in the literature in the use of the terms blue, purple, and violet. Parsons[32] speaks of violet as a color "which occurs rarely in nature." It is certainly true that a color composed only of the shortest wave-lengths is very rare in nature (in the rainbow and on a few butterflies), but to my own eye there are 'purples' which exactly match it but are spectrally mixtures of blue or violet and red. Evidently to Parsons there is a far smaller proportion of red in the sensation produced by the shortest waves, for he writes as if there were little likelihood of confusing violet (as a color) with purple, and as though the confusion were only of nomenclature.[33] One the other hand he accepts Abney's 'blue' of 460 mμ, and even 435 mμ,[34] as blues without comment. To my own eye neither has any real right to the name of blue, the first is decidedly an 'indigo' half way between blue and violet, the second has almost as much red as blue, and could be well matched by the 'purple' of such flowers as the clematis or the violet. My belief is that such variation of judgment points to a real variation in the amount of red stimulated by the short waves, and by the theory here presented is easily explained on the ground that persons who compare spectral violet with blue rather than with purple have a substantially thinner layer of pigment than those (like myself) to whom 'violet' and 'purple' are merely different words. On the classical theories this variation is difficult to explain, as an abnormal amount of yellow pigment in the macula should produce more disturbance of the blue (480mμ) as well.

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[31] I propose to use the term *mesopic* to represent the states of eye adaptation intermediate between the ordinary daylight state (photopic) and the fully achromatic state of dark-adaptation (scotopic). Besides qualities intermediate between those of day and dark adaptation it has some special features of its own, notably the dominance of the Purkinje type of flash after-image, and a peculiarly high degree of saturation of some colors, combined with a relative insensitiveness for yellow. In full photopic vision the Hering after-image is dominant or exists alone, while in scotopic vision no colored after-images are present. A further and more important reason for distinguishing this level, is that a very large part of the actual experimental work is done within it, the stimuli being kept for convenience near the lowest level at which colors are perceived with good saturation, and this work has been reported either as done on photopic or on scotopic vision according as the observer had lately been working in daylight or in a dark-room. This is also a region where the eye both fatigues and adapts with maximum rapidity, so that the exact state of the eye is far more important than at either scotopic or photopic levels. It may be roughly defined as the period after 12 min. in the dark-room and until color sense is lost. Further on (Fig. 5) I have tried to indicate some possibilities of further subdivisions of this series of states.

[32] *Op. cit.*, 27.

[33] *Op. cit.*, 75.

[34] *Op. cit.*, 163, in discussion of König and Dieterici's work.

Brightness and saturation. If my theory is true, brilliance or luminosity,[35] as measured for instance by the flicker photometer, is a function of the white organs *only*, as with Hering's theory.

By the usual method of use of the flicker photometer the rapidity of alternation of the two fields compared is set so that there is a strong sense of flicker when white lights of unequal brightness are compared. If then we compare two contrastingly *colored* lights of slightly unequal brightness there is a curious phenomenon. There is first a well-marked flicker of the same frequency as before, but without any distinct color quality, and superposed on this a very marked but slow alternation of the two contrasting *colors* in practically their full saturation or even with exaggerated saturation. This shows very strikingly with green and orange. At the proper degree of unbalance I am able to see both the rapid flicker and the slower alternation. If my theory (or any other which involves a separate white process) is true, the true flicker is due to the white-organs alone, and is entirely independent of the slower alternation which would be due to the more strongly stimulated pair of color-organs. The slow alternation would no doubt be some form of fatigue-phenomenon. At a slower speed the apparent alternation of color is synchronized with the actual alternation of stimuli, but at that speed the noticeable flicker of *intensity* is no longer prominent. The simplest explanation would be that the white- and color-organs have different rates of flicker, and the alternation which causes the phenomenon in either is ineffective or produces a non-synchronous fatigue effect in the other. If so the flicker photometer as practically used would be a really accurate measure of the effect of the various lights on the white-organs *alone*.

There is a good deal of confusion in the use of the terms brightness, brilliancy, luminosity, and the like. We may accept Troland's definitions: brilliancy is the psychological effect, which according to the four-color theories measures the activity of the white-process, and is what Abney and others call luminosity. Luminosity will be the quasi-physical property which would correspond to the brilliancy as seen by an eye in a standard state of adaptation, and is a practically measurable quantity, by comparison under standard conditions with a standard light. Brilliancy, according to this theory, is luminosity multiplied by a factor representing the amount of functional visual pigment at a given moment.

Similarly there are two different conceptions of saturation. First, it may be viewed as the measurable ratio between the amount of spectral light and the total (spectral *plus* white) light in an artificial mixture of spectral and white light which matches the given light (this is a quasi-physical quantity corresponding to luminosity, and applies to a standard eye). Secondly it may be viewed as the ratio of the activity of the color process *in the eye* to the white process (brilliance). There is no necessary relation of one of these things to the other, but I have not seen them clearly distinguished. Under the present theory the activity of the color process, being due to a state of balance of the two sides of the color cones, will tend to become weaker with exhaustion of the visual pigment by the total amount of light, while in equilibrium the white process tends to become unity (as explained in the first part of this section). Therefore the saturation (sensation) will tend to become less with increasing luminosity, without any change in the physical saturation. If the eye is not in equilibrium the brilliance may be more or less than unity, according as the stimulus

[35] The words luminosity and brilliance are distinguished and defined by Troland (*J. Opt. Soc. Amer.*, 6, 1922, 537), restricting brilliance to the meaning given here, which is called luminosity by Parsons (*op. cit.*, 29), Abney, and others. Troland would restrict the term luminosity to the psychophysical quality mentioned below.

has lately increased or decreased, and the color process will also increase or decrease *pari passu*, if there has not been time for any adaptation, but if adaptation has begun the effect will depend on the relative adaptation curves of the white and color organs, and cannot be predicted a priori. But considering the apparent sluggishness of the color organs, as shown by

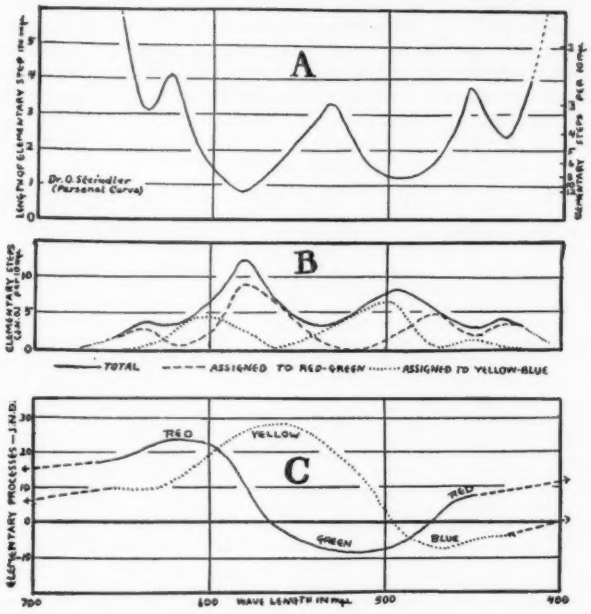

FIG. 4. Elementary processes, plotted in J.N.D. units.

A shows Steindler's data on the minimum discriminable difference of wave-length for the various hues of the spectrum. The scale at the left shows the length of the elementary step (J.N.D.) in millimicrons, the scale at the right the corresponding number of elementary steps per 10 mμ.

B shows the same data replotted with the number of J.N.D. per 10 mμ as a unit, and these numbers are arbitrarily divided between the red-green and yellow-blue processes, on the assumption that they share equally in accounting for hue-discrimination as a whole, and that the sum of the steps of discrimination for which each is responsible in a given interval is the number of J.N.D.s observed in that interval. It is assumed that at the two points where protanopes show minima of hue (saturation) discrimination, the whole normal discriminating power is to be assigned to the red-green process, and that there exist two similar points where the whole discrimination is due to the blue-yellow process, these being the points of invariant blue and yellow.

C is constructed by measuring off the numbers of steps thus found in the second curve.

their relative slowness of flicker, we may expect a decrease of sensation-saturation with a partially adapted increase in intensity, and an increase with a decrease in intensity. Of course with very low intensities the color organs drop out and we get decreasing sensation-saturation under a constant stimulus-saturation, even in equilibrium.

Discriminability of hues. Steindler[36] and others have found the power of discrimination of hues very different in different parts of the spectrum and substantially different even for different individuals. Steindler corrects for difference of brightness, but seems to have made no attempt to distinguish between difference in hue and in saturation. It is to be assumed that with color-blind individuals saturation alone is involved, but that with normals both are present.

I present, in Fig. 4, Steindler's data for herself, as a curve, and I attempt to analyse it into the two color-processes, on the assumption that the number of discriminable steps between any two colors is the sum of the unit differences for the two color processes between the same limits. For some of the observers a smoother curve could be made by assuming that except where the two color processes were of substantially equal strength, only one of them was active in discrimination. The relative smoothness of the two unitary curves, as compared with the combined (experimental) curve of discriminating power, is striking. To get the general character of my blue-yellow curve I have chosen those of her protanopic observers who had about half the normal number of steps in the spectrum, and I have allowed about 24 steps to make the connection between violet and red, not studied by her. If the exact neutral points of individual observers were available it might be possible to make even a better curve, as presumably these should fix the points where the two curves cross the base line. The very unequal number of steps between complementary colors according as one goes through yellow and green or through purple, indicates that the discriminating power in the yellow and green is in some way dulled. Perhaps the intensity of the white process in that range lessens the discriminating power for colors. Presumably all these experiments were done in the 'mesopic' range of the eye,[37] and part of this buried discriminating power might be uncovered at a higher adaptation level, especially in the yellow. As the average primary colors were also determined with weak lights, there is probably no extra error introduced in using them. I have used a red ¼ way to violet, yellow 575 mμ, green 500 mμ, and blue 475 mμ, being a fair average of the values determined by the peripheral retina, by introspective methods, and by the protanopic eye, which I assume has the blue-yellow process unharmed.

Night-blindness. There seem to be two distinct phenomena called night-blindness. In the more typical case the sensitivity of the retina becomes zero at a little below the level where for a normal person scotopic vision comes in. This type can be easily explained as due merely to the complete absence of the scotopic pigment. If that is the case there will be a zone where the eye can still perceive light, but much less than in the case of a normal eye, this being the zone in which the photopic pigment is still sensitive, but in the normal eye is overlapped and covered up by the vastly greater sensitiveness

[36] O. Steindler, *Sitz. d. Wiener Akad.*, 115, 1906, 115; Parsons, *op. cit.*, 30.
[37] See note 31, above.

of the scotopic; and a lower zone where in the normal eye the scotopic pigment alone functions. There would not necessarily be any change in the color threshold. Another possibility would be a subnormal quantity merely, of the scotopic pigment. Thirdly, there might be a subnormal supply of the restoring substance, either from actual lack of material or from deficient circulation. In this case, as in some known night-blind individuals, there should be slow adaptation, without necessarily any change in the final level of adaptation.

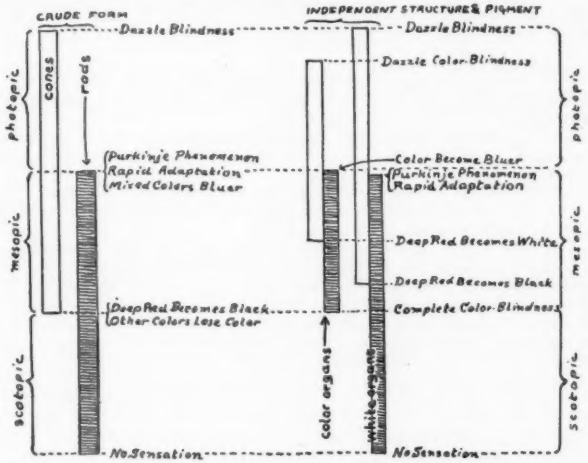

Fig. 5. Diagrams of the duplicity theory and its various thresholds, the left diagram on the usual basis, the right one on the assumption that structure (color process) and pigment (photopic and scotopic) are independent.

Thresholds. If in fact there are two types of end-organs, one of which at least is provided with two different pigments, there should be three or even four separate thresholds, representing where the color function changes to scotopic and then drops out, and where the white-organs do the same. Further, if the extreme red light is used, to which the scotopic pigment is insensitive, the two bottom thresholds of the photopic process might be identified.

In the ordinary form of the 'duplicity theory' (Fig. 5) it is assumed (often tacitly) that the two forms of end organs are wholly distinct, in which case there should be a single threshold where the change of function of one to the other type occurs, or at most two, with a region between them where both types are

functioning together; and most formulations are on the basis of a single threshold. If light sense and color sense are independent this is by no means necessary. In fact, there is a well marked change in the color sense, marked most conspicuously by a decreasing yellow sensation in all colors of which yellow is a component, at a considerably higher level than that at which color drops out. This change seems to take place at the same level where with a momentary stimulus the Purkinje type of after-image takes the place of the Hering type, while the whole range of conditions where the Purkinje image is alone visible seems unprovided for by the usual theories.

The thresholds then are:
(1) Color becomes scotopic—Hering after-image drops out with moderately dazzling stimuli, yellow sensation weakens, deep red light appears white;[38]
(2) White sense becomes scotopic—adaptation suddenly becomes very rapid (12 min. in the dark), Purkinje phenomenon (relative insensitiveness for the red end of spectrum);
(3) Photopic white-sense drops out—deep red becomes black;
(4) Color sense becomes blind—all color sense vanishes, after-images also becoming colorless;
(5) White sense becomes blind and the eye is wholly insensitive.
These thresholds are obviously different in different zones of the retina. In the center of the fovea they are apparently at the same level, while toward the periphery, on account of the small number of color-organs, in the case of light of low intensity or saturation, the white sense overcomes the color before the latter is theoretically extinguished. I think all five thresholds have been identified in the outer parafoveal region.

Retinal color-fields.[39] One of the strongest supports of a two-color and white theory is the existence of four and only four colors (presumably the primaries) which do not change color as one passes out to the peripheral retina. These are identified by Hess and others[40] as an ultraspectral red (complentary to 495 mμ), yellow 574.5 or 570 mμ, green about 495 mμ, and blue 460-470 mμ. There is substantial agreement, especially as in general no allowance is made for eye adaptation—threshold (1) above— or for foveal pigmentation, and in some cases even fatigue does not seem to have been clearly eliminated.

Abney[41] stands out in contradiction to these results, as he finds no sharp color boundary in the outer retina; but he chose colors which were known not to be the invariant ones, and does not state whether his setting is the threshold for color, or the point of color change. He gets markedly similar curves for his 'red' and 'yellow,' which is hardly surprising when one considers that his 'red' was a spectral color, which is generally admitted to stimulate the yellow (or on the three-color formulation the green) sense, and his 'yellow' was an orange with a conspicuous percent of red (the sodium line). His green was definitely more yellow than the invariant color, and in his more careful experiments was strongly yellow-

[38] Parsons, *op. cit.*, 61.
[39] Parsons, *op. cit.*, 75 ff.
[40] Parsons, *op. cit.*, 68-70 gives references.
[41] W. de W. Abney, *Phil. Trans. R .S.*, 190 A, 1897, 155-195; *Researches in Colour Vision*, 190; Parsons, *op. cit.*, 74-80.

ish (530 mμ) while his blue, at first not far from the invariant color, was later a true and pure violet (430 mμ).

After-images.[42] The generally recognized after-images of a short stimulus are of three types: the Hering is momentary, and positive both in hue and brilliance; the Purkinje is positive in brilliance but negative in hue; and the slow image is positive in brilliance, faintly positive in hue, and diffuse. By the present theory the color and white processes should be considered separately. After a momentary colored stimulus at a high adaptation level there should be in the white-organs an accumulation of the activating substance and partial exhaustion but a steady replenishment of the sensitive pigment. So long as the activating substance is undischarged there will be stimulus of the nerve-ends, and a brightness-sensation. I assume that the discharge takes a perceptible length of time, and longer as the amount of substance (which is dependent on the *product* of the amount of light by the amount of visual pigment at the moment of stimulus) is greater. If the activating substance is discharged before the exhausted pigment is restored to an average level, there will be a lowering of the idioretinal activity in darkness or of the sensitivity in light, and a negative after-image will result. Finally the third after-image, which is diffuse and shows the appearance of scotopic vision, is, I assume, dependent on reflex activity, and also possibly on some oscillation in the relation of the process of restoration and of idioretinal discharge.

If the stimulating light is colored, there will also be a similar state of the color-organs. In this case the pigment will be not bleached in the organ as a whole, but in a zone which throws the organ out of equilibrium. Suppose the stimulus light is orange. Then in the deeper side of the color organs there will be a region of accumulation of the active substance ('acid'), which will cause the organ to function positively for a greater or less period according to the amount accumulated, just as with the white-organs. When this is dicharged there may come on a negative period, just as with the white-organs, but not necessarily synchronous with it. If the negative period comes on while the white-organs are still positive there will be a Purkinje (− color, + white) following the Hering image; if they are simultaneous, the negative phase will be superposed on a black sensation and will be difficult to detect, though I have seen it, especially with a red stimulus.

If the stimulus were blue or green the effect would be reversed, inasmuch as the accumulated substance would cause inhibition, and when it was discharged the remaining unbalance would leave activity on the deep, positive side of the cone.

In my own case a flash of moderate intensity with day adaptation produces only a positive (Hering) flash; in this case no doubt the color process is restored before the active substance is exhausted, or else the effect comes after the white-organ has reached its negative phase and is overlooked. In the case of a very intense stimulus, both the activity of the white and the exhaustion are enormously increased, and after a Hering phase a period comes when the cone is negative and the white-organ positive, and the Purkinje image appears.

[42]Parsons, *op. cit.*, 42 ff.; D. B. Judd, *op. cit.*

The flight of colors, which appears with a particularly intense stimulus, would appear to be due to the two sets of color organs having different periods. It is said to lack the yellow element,[43] but in my own case the yellow is very easily elicited, much more easily than green. Of course if the yellow falls on a negative condition of the white-organ it will be very dark and will appear as a brown. This is obviously dependent on the relative rate of oscillation of the color- and white-organs, and can be adjusted in a given person by changing the saturation, decreasing saturation naturally causing a relative greater stimulation of the white process and a slower relative oscillation.

I assume that if the cones are wholly saturated with pigment under dark adaptation the restoring process comes to rest. In that case a colored stimulus will produce a minimum of active substance, which will be soon discharged, while the restoring process, having been dormant just before the stimulus, will be slow in taking effect; therefore there will be a minimal and sub-threshold positive (Hering) image, but a marked negative (Purkinje) one. I take then the presence of the Purkinje image alone with a moderate stimulus as a sign that the cones at the moment of stimulus were saturated, and restoration inactive. With a very bright light, of course, there will be enough substance decomposed to persist and leave a Hering image preceding the Purkinje one.

Color Blindness.[44] As in other two-color theories I assume that the protanope merely lacks the red-green process and organs, being otherwise either normal, or possibly in some cases also lacking the visual violet, but not the blue-yellow organs or structures at the fovea. In that case the spectrum will be bisected at the neutral point for the blue-yellow organs, all wave-lengths below that giving blue and above it yellow. This should give a value for the neutral point the same as the criterion of invariance in a normal person and does so within the possible range of accuracy (498 mμ).[45]

I assume that in the case of the deuteranope there has been, not a loss of organs but a reversion of type in the color organs, so that there is a single type intermediate in properties between the usual two. There seems to be a slight change in the neutral point (about 5 mμ in the case of Steindler's measurements, where the two series seem to have been strictly comparable), and the sensitivity to the red end is markedly shortened, and would appear to correspond to the 'violet-blind' individual with no other defect, the layer of pigment in the color-cones not being deep enough to produce a red element in the violet sensation, (perhaps I should rather say a warm element in the extreme cold sensation). Whether the white-sensation is or is not abnormal in these two types should be determinable by a study of lights of varying saturation, which I think has not been done.

The tritanope would simply lack the blue-yellow organs. It is interesting that these should according to this structure be thinner in their 'working-parts' than the other sets, and so more susceptible, perhaps, to

[43] McDougall, as quoted by Parsons, *op. cit.*, III, 277 ff.
[44] Parsons, *op. cit.*, 169 ff.
[45] Steindler, *op. cit.*

injury from the back of the retina. It seems in fact that tritanopia is wholly the result of a pathological process, and, normally at least, of temporary separation of the retina.[46]

Complete color blindness would be primarily due to a lack of color organs of either type. It is a very rare condition and usually seems to be correlated with other pathological changes. If this theory is true there should be a possibility of cases where the color blindness exists without the usual accompanying day-blindness due presumably to the lack of the 'visual violet.' I do not know of any such cases.

Summary

The two leading theories of color vision at present are (1) the *three-color* theory, which is insufficient to psychologists as indicated by Ladd-Franklin and others; and (2) the *four-color*, which is unsatisfactory to physicists. Ladd-Franklin's compromise theory is also unsatisfactory because it postulates at least three separate processes in a single nerve fibre.

One nerve fibre can carry only one excitation, variable in only one direction, *i.e.* frequency. Three separate variables are necessary to supply the known range of color. Three nerves are, therefore, needed for a complete color sensation. As there is only one nerve fibre to every cone, three kinds of cones are required for theory.

The interference theory here proposed assumes that only one light-sensitive substance is concerned with photopic vision—a visual violet substance similar to the familiar visual purple of scotopic vision.

Three kinds of cones, however, are postulated: (1) *white*, with no special structures, in which the visual pigment is presumably diffuse; (2) *yellow*, with a reflecting surface at its further end and an insulating film at a distance back from it where interference causes a maximum intensity for *green*—the visual pigment being present in equal amounts on the two sides of the film; and (3) *red*, similar to the yellow in structure, but with the insulating film at the maximum level for yellow.

White light and light which fails of reflection produce equal activity on both sides of the film and no net action on the color cones.

Colored light has a level of maximum activity caused by interference of the direct and reflected waves. If this lies on the same side of the film as cell-body and nerve it causes increased activity of the nerve (above idioretinal level) and yellow or red sensation. If beyond the film it causes inhibition, and the blue and green sensations respectively. Oxidation, with its resultant acid formation and change of colloidal state, is tentatively suggested as a possible mechanism—acidity in contact with the nerve causing activity, and acidity away from the nerve producing a relative basic condition next the nerve and inhibition.

Visual pigment is restored at a relatively constant rate but ceases under a very dim light as the cones become saturated. Under constant illumination the pigment is exhausted to a point of equilibrium—to a point where it is restored as rapidly as it is exhausted. The color-cones tend, therefore, to produce a uniform color sense under any illumination if the ratio of unbalanced light (colored) to the total remains constant. (This may be somewhat modified by the reflex control of the restoring substance.)

The total capacity of white-cones is higher than that of the colored. Very dim light, therefore, produces no color-sensation.

Polarization of the cones is not discharged immediately; the lag causes the positive after-image. Purple is restored slowly, causing an unbalanced idioretinal activity and a negative after-image.

The exposition of the interference theory presented here is followed by a discussion of other theories and of published data.

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[46]See A. König, *Sitz. k. Pr. Akad. Wiss. zu Berlin*, 1897, 718 (collected apers, p. 396)-Parsons, *op. cit.*, 180-181, gives a full bibliography.

ON SUBSTITUTION AS A CAUSE OF ERRORS IN THINKING

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I

The aim of this study is to examine critically Selz's theory of errors in thinking.[1] On the positive side, Selz explains how errors occur; on the negative side he refutes the theory which depends upon the law of substitution. We intend in this paper to show that Selz's criticisms are not sound; that the law of substitution, properly understood, is valid; and furthermore, that the law of substitution provides, for at least one type of error, a necessary supplement to the explanation given by Selz.

Selz's own theory is based on the fact that, in the case of task-directed thinking, we really have in the *Aufgabe* a conscious aim before us, and therefore anticipate the solution. But the anticipation is not complete, *i.e.* it does not cover the solution sought for in all respects, such as might occur when we anticipate an event by means of an image of it. The anticipation of an aim of thought, of the solution of a present problem, is, on the contrary, a schematic anticipation. All the constituents of the result sought for are anticipated in some way or other. One of these constituents is, however, anticipated more or less indefinitely, viz., that which has to be found by the process of thought itself. But this indefiniteness of the constituent or part sought concerns only that part itself, not its relations to the other constituents of the solution. These are already anticipated in the consciousness of the aim. The solution itself forms a relational whole (*Sachverhältnis*). In the anticipation of the relations of the constituent sought to the already known constituents of this related whole, we have psychologically, in the consciousness of the aim, the indirect characterization of what is sought; for, because the anticipation is thus constituted, only one object will correspond to it as standing in these relations, and hence only one such object can be recognized as agreeing with the anticipation and as the solution of the problem or task.[2]

*Accepted for publication February 20, 1927.

[1] Otto Selz, *Über die Gesetze des geordneten Denkverlaufs*, 1913; *Zur Psychologie des Produktiven Denkens und des Irrtums*, 1922. These titles will hereafter be referred to respectively as Selz I and II.

[2] Selz, II, 371.

Arguing from the introspective protocols obtained from his subjects, as well as along other lines to be discussed later, Selz comes to the conclusion that incorrect reactions of the *Ss* to the problems set them, were not due to strong reproductive tendencies issuing from the stimulus-word and running contrary to the direction of the task, but that such reactions were the result, at least usually, of task-directed intellectual operations serving the solution of the problem. The most common ground for these errors was, in his experiments, that a related problem was served instead of the one actually set. They were thus due to what may be called task-displacement.[3] Thus, the task actually set may be misunderstood; or, without the actual occurrence of a misunderstanding, only a part of the task may exercise sufficient directive influence on the ensuing processes of thought, some of the characteristics which the correct solution would have to satisfy being lost sight of to a greater or less extent.[4] In some cases, where a particular characteristic of the task remained too inactive, another took its place, the fact being unnoticed. In others, the characteristic was merely dropped, instead of being displaced by another.[5] The mistakes, in a further class of cases, were not due to the fact that the task undertaken, while clearly conceived, was understood in a sense related to but not identical with that of the task actually set. They were, on the contrary, due to the fact that the task set was not clearly thought, and so was merely shifted in the direction of a related one. Such lack of clarity with regard to the task may consist in the fact that only one or more of the characteristics of the task set come into the foreground of consciousness, while others have too low a degree of consciousness or of dispositional efficiency.

On the basis of the types of data described above, Selz formulates the law of complex-activity[6] or efficiency as follows: The lower the degree of consciousness or of dispositional efficiency of constituents (independent or dependent) of a complex, the more readily do these constituents remain without influence in the activity of the complex, *i.e.* the more readily the complex acts as if these independent or dependent constituents were not present. He calls this the *law of part-efficiency of complexes*. On the other hand, if a complex is acting with full efficiency, then all its dependent or independent constituents exert a directive influence as a whole on the ensuing process. The partial activity of a task, being thus due to task-displacement, takes place in the same way as does the full activity of the task. It leads to errors only because not all parts of the task actually set are sufficiently active.[7] If, however, the thinker has at his disposal a

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[3] Selz, II, 1 f. Selz speaks of *Aufgabevertauschung*, *i.e.* task-substitution. Since we shall, however, have to make use of the word 'substitution' in a special sense, we prefer the term given in the text for purposes of our discussion.

[4] Selz, II, 499.

[5] Selz, II, 32.

[6] According to Selz, the anticipation of the solution forms a complex in the sense of the consciousness of a whole, including the consciousness of the relations between the parts. Independent parts of a task are those which, taken by themselves, would form a full task and which may drop out without the meaning or psychological efficiency of the other parts being affected. Dependent parts are those which alone would form no full task, or, if they do so, cannot be dropped without altering the sense of the rest. Selz, II, 2.

[7] Selz II, 10, 12. "Je geringer der Bewusstseinsgrad bzw. der dispositionelle Wirkungsgrad von Bestandstücken (selbständigen Teilen oder unselbständigen Momenten) eines Komplexes ist, desto eher bleiben diese Bestandstücke ohne Einfluss auf die Komplexwirkung, d.h., um so eher wirkt der Komplex so, als ob jene selbständigen oder unselbständigen Bestandstücke nicht vorhanden wären. . . . Das Gesetz der Teilwirksamkeit von Komplexen. . . . Die Teilwirksamkeit einer Aufgabe aber vollzieht sich nach denselben Gesetzmässigkeiten wie eine volle Aufgabenwirksamkeit, sie muss nur zu Fehlreaktionen führen, weil bei ihr nicht alle Bestandstücke der Aufgabe zur Geltung kommen."

We are translating Selz's *dispositionelle Wirkungsgrad* by "dispositional efficiency." Selz contrasts the degree of consciousness of the task with its dispositional efficiency. We take "dispositional efficiency of the task" as meaning the directive influence which the task exerts on the ensuing process of thought at a time when the task is *no longer* in consciousness.

familiar solution, then a constituent of the task, having a low degree of consciousness, can still have influence on his reaction. Should such a solution not be at his disposal, reproductions will take place which correspond only to the other characteristics of the task.[8] Furthermore, if one task is displaced by another, the latter will, of course, in its turn form a complex in the sense defined above. If we give the name of residual complex to that part of the original or total complex which still retains enough efficiency, then partial activity of the original complex will take place the more readily, the stronger the tendencies are which proceed from the residual complex (as a whole) in comparison with the strength of the tendencies proceeding from the total complex. This part-efficiency may be the result, for example, of the familiarity of the solution corresponding to the *fully active* constituents of the anticipatory complex schema.[9] Where the meaning of a task is not clearly apprehended, that meaning of it may obtain the upper hand which offers an easier solution as the result of being more familiar.[10] Thus the task 'whole' displaced the task 'superordinate' only when the former offered an easier solution.[11] A similar phenomenon occurs in the case of motor activities.[12] In other cases a shifting of the meaning of the stimulus-word occurs, *i.e.* it is displaced in consciousness by another meaning closely related to it. Since the total task includes the meaning of the stimulus-word, we have to do in such cases with a part-efficiency of the total task. Favorable and contributory to this taking place is a fragmentary imaginal representation of the meaning of the stimulus-word, or also the fact that its meaning is present in an exemplificatory way, *e.g.* as when we think of a special case either in an imaginal or non-imaginal manner.[13] Every distraction of attention favors lack of clarity with regard to particular characteristics of the task. Especially if a part of the task has already a lower degree of consciousness than the others, a reduction of the degree of clearness of the task must affect such a part most of all.[14] Selz holds, furthermore, that the more abstract characteristics of the objects of which we have knowledge are, often at least, more readily brought to consciousness than are their more concrete features. Where a content comes to consciousness in isolation, or relative isolation, from others with which it is connected in our knowledge, we may say that it is abstracted from the full content. The abstract characteristics will, then, have a higher degree of abstractive preparedness.[15] If then the abstract characteristics come most easily to consciousness, *i.e.* if among the active constituents of the meaning of a task the most abstract characteristics have usually the highest degree of consciousness or efficiency, it is possible to see why the great majority of errors in the experiments were conditioned by a partial efficiency of the task. This partial efficiency was in all cases traced back by Selz to the fact that the more abstract or general characteristics possessed a higher degree of consciousness or of dispositional efficiency than the more special. Hence, according to the law of part-efficiency, the former acted alone. If, then, distractions of attention take place; if the observer is restless or hurried; if he underestimates the difficulty of the task, then these factors, in reducing the degree of consciousness or the degree of dispositional efficiency of the complex for the total task, must increase the danger that only the more general, more strongly present, characteristics of the task make themselves felt in the ensuing process. On this hypothesis we can explain how it is that these special conditions just mentioned are instrumental in bringing about errors.

[8] Selz, II, 25.
[9] *Ibid.*, 10 f.
[10] *Ibid.*, 6.
[11] *Ibid.*, 12-3.
[12] *Ibid.*, 572.
[13] *Ibid.*, 35-36.
[14] *Ibid.*, 15.
[15] Selz's *Abstraktionsbereitschaft.*

In so far as the partial efficiency of the task is a result of the higher degree of consciousness of the task's most general characteristics, this higher degree of consciousness is itself the result of the superior abstractive preparedness of the most general characteristics of an active meaning-complex. The consequence is that these characteristics tend to determine the ensuing process according to the law of part-efficiency. In so far as the difference between the more active general characteristics and the special characteristics of the task's meaning is not a difference of degree of consciousness, but only a difference of dispositional efficiency, we have to do with a special case of difference of reproductive preparedness. As in all cases of such differences between parts of a content with regard to which abstraction can take place, this difference of reproductive preparedness implies a corresponding difference of abstractive preparedness.[16]

For the present the foregoing statement of Selz's own theory may suffice. It follows closely (in translation) his own words. We may merely remark that the factors he advances, do, according to our view, play an important rôle in causing errors in thinking. The question is, however, whether they hold to the exclusion of factors acting according to the law of substitution—the validity of which is questioned by Selz. We now turn to the discussion of these further possible factors.

II

A second theory, suggested but rejected by Selz, of the explanation of errors in thinking may be obtained by the application of the law of substitution.

Müller and Pilzecker formulate the law of substitution as follows. If an idea a arises in consciousness according to one or other of the laws of association, and if there is connected with this idea a tendency to reproduce the idea b, then this tendency may forthwith (*ohne weiteres*) be effective also for the arousal of the idea α which is only similar to a; the idea α can, so to speak, be substituted in the association ab for the similar idea a. The probability that α will reproduce b is greater, the more similar α is to a, the firmer the association is between a and b, and the less α is itself associated with any other ideas. In the same way a reproductive tendency attached to an ideational complex may forthwith be transferred to another ideational complex which is only similar to the first. A process of this nature is called active substitution. In the case of passive substitution the idea a does not reproduce an idea b which is actually associated with it, but reproduces an idea β which is similar to b, with which a, however, is apparently not associated.[17]

In carrying out experiments on memory, Müller and Pilzecker came to the conclusion that a considerable portion of the errors in reproduction that they observed were due to substitutions falling under this law. Passive substitution occurred especially where the incorrect syllable was more familiar (*geläufig*) than the correct one, or had occurred recently in the experiments. Possibly in some cases of substitution the substituted syllable actually came to consciousness in uttering the correct syllable when the series of syllables was being read. On account of superior reproductive preparedness it would then, at testing, be reproduced instead of the cor-

[16] Selz, II, 498 ff.

[17] G. E. Müller and A. Pilzecker, Experimentelle Beiträge zur Lehre vom Gedächtnis, *Zsch. f. Psychol.*, Erg. Bd. 1, 1900, 212-16. The law, as formulated by these writers, specifies the arousal of the reproducing idea according to one or other of the laws of association. This limitation is not essential for purposes of our discussion.

rect syllable. In some cases active and passive substitution occurred together, superior reproductive preparedness of the substituted syllable with which the reaction took place, playing a similar rôle as in the type of cases mentioned above.

If, now, syllables have been presented to a subject in a certain temporal and spatial order of which the subject has become aware, then the syllables would have become associated with each other in our dispositional knowledge[18] of them as standing in these relations to one another, *i.e.* as forming a relational whole. On the basis of this dispositional knowledge later reproduction can take place.

In attempting now, to reproduce the second of a pair of syllables, the first being given, we are carrying out a task-directed process of thought. The anticipation is primarily characterized as "the syllable ■ coming immediately after the syllable *a* in the series." According to Selz's third law of complex-completion, the basal condition is now fulfilled for bringing the content anticipated into consciousness. This law states that an undertaking (*Determination*) directed to the completion of a schematically anticipated complex is causal for the reproduction of the whole complex.[19]

An *S* may be attempting to reproduce the second of two syllables and one of its letters may perhaps be brought to consciousness at first. Should the dispositional knowledge for another syllable also containing this letter, *e.g.* in the same place, be in a state of sufficiently superior reproductive preparedness, then this syllable would be reproduced instead of the correct one. Cases of this nature are reported by Müller and Pilzecker.[20] Clearly the correspondence between the fuller anticipation, arrived at by means of the reproduction of the common letter, would be an important factor for bringing about the substitution. It is, however, not necessary that a common letter need be brought to consciousness at first for substitution to take place, since other points of agreement between anticipation and substituted syllable may suffice, *e.g.* that consisting of two consonants and a vowel. It is clear, however, that the degree of similarity between the substituting and the correct content is of importance for bringing about an incorrect reproduction. The correct content is characterized schematically in the anticipation. The greater the correspondence between these characteristics of the correct content and any other one for which dispositional knowledge exists, the greater the chances, other things being equal, for a substitution to take place. The probability that the anticipation will correspond, in part, at any rate, to another content will clearly increase with the increase in correspondence between the incorrect and the correct one.

We have up to now, however, adduced only one reason for the necessity of a certain degree of correspondence between anticipation and substituted content, namely that referring to the possibility of the *reproduction* of the latter. Another factor comes into play, however, as soon as the substitution is carried so far that the error is not *recognized*. If the reproduced content disagrees, for the *S*, with the characteristics definitely given to the correct one in the anticipation in such a way that this disagreement can be recognized, the reproduced content will not be looked upon as correct. Similarly, for the substituted syllable to be looked upon as correct presupposes that the two were, for the *S*, similar to one another. Furthermore, in so far as lack of associations between the substituted and other contents decreases the chances of knowledge about it being reproduced, which stands in disagreement with the anticipation, lack of such associations becomes a favorable condition for the non-recognition of errors. Should such further knowledge not be reproduced, then the substituted content may be looked

[18] Selz's *Wissensdispositionen*, I, 154.
[19] Selz, I, 128.
[20] *Op. cit.*, 215.

upon as correct, or as possibly correct, partly on the basis of such criteria of recollective correctness as the promptness or clearness with which it is reproduced,[21] partly because it has not, as reproduced, any characteristics standing in definite disagreement with the anticipation, but especially because it stands in positive agreement with the anticipation in some respects.

The fact, however, that a certain degree of correspondence must have existed between anticipation and substituted content does not imply that the anticipatory conditions were equally favorable for the substituted and the correct content. Whenever the substituted content possesses less agreement with the anticipation than the correct content does, then the anticipatory conditions must in themselves have been more favorable for the reproduction of the correct content. The reason for the incorrect reproduction in such cases cannot thus, be found only in the anticipatory conditions, and we have to accept the presence of another factor for bringing about the substitution, such as, *e.g.* a relatively high degree of reproductive preparedness of at least a part of the dispositional knowledge for the content substituted.

The view here developed with regard to the mechanism according to which passive substitution takes place, at the same time explains a peculiarity of some of the cases which Müller and Pilzecker adduce. It might appear remarkable that so small a degree of similarity between the correct and the substituted syllable should form the basis for the occurrence of the error. In itself indeed this, or even a much larger, degree of similarity would not suffice. There is necessary, in addition, a sufficiently superior degree of reproductive preparedness of the syllable reproduced to overcome the disadvantage due to the anticipatory conditions.

We have now to proceed to the examination of the meaning of the law of active substitution and of its relation to that of passive substitution. We presuppose that the syllables a' and b' have been associated with each other as standing in the relationship of succession r'. The syllable a'' has furthermore been associated with the syllable z as standing to it in the relationship r'. Should the task now be undertaken of recalling the syllable following on a'', the anticipation so characterised may lead to the reproduction of b'. If we formulate in this way the primary conditions for the occurrence of the phenomena subsumed by Müller and Pilzecker under the heading of active substitution, it is clear that the following conditions will be favorable to the reproduction of the incorrect syllable.

(a) Lack of reproductive preparedness of the content z itself which is associated with a'' as standing in the relation r' to it, and weakness of the association between a'' and z.

(b) Relative prominence in the anticipatory consciousness of those characteristics of a'' with regard to which it corresponds with a', so that the anticipation approaches (although, by definition, it does not coincide completely with) that anticipation which is directed to the recall of this syllable succeeding a'. Other things being equal, the chances of this happening increase with the increase in similarity between a' and a''. This second condition is that of a partial efficiency of the anticipation directed towards the reproduction of z.[22]

(c) Should the condition given under (b) be realised, then the strength of the association between a' and b' as standing in the relation r', and high reproductive preparedness of b' itself, will be further factors favorable for the reproduction of b'. The chances for this reproduction taking place will thus increase with the increase of the total dispositional preparedness

[21] G. E. Müller, Zur Analyse der Gedächtnistätigkeit und des Vorstellungsverlaufs, *Zsch. f. Psych.*, Erg. Bd. 8, 1915, 224-240.

[22] Cf., however, Section IV, below.

accruing to b', which will in this case depend not only on the reproductive preparedness of b' itself, but also on the strength of its association with a'.[22]

In the cases of active substitution different classes of cases falling under the same general type have to be distinguished. (1) a' and b' are associated with each other in the relation r'. So, too, a'' and b'' are associated with each other as standing in the same or in a similar relation to each other. We now set to work with an anticipation which we may symbolize by means of $\frac{r'}{a''}$ and which is directed towards the reproduction of b''. Should b' now be reproduced we should have to call this a process of active substitution, since it is a', and not a'', which is associated with b'. We could, however, equally well call it passive substitution from the point of view that b' has been substituted for the similar content b''. In this type of case, then, the substitution does not take place on the basis of a direct association between a'' and b', and the chances for substitution taking place will be favored by the degree to which the characteristics prominent in the anticipatory consciousness agree with those in the anticipation $\frac{r'}{a'}$ directed towards the reproduction of b'. (2) Müller and Pilzecker include under the cases of passive substitution those in which direct associations between the substituted syllable and the one presented at testing may have been in existence. We may now suppose that, in the generalized cases we are here discussing, a'' has actually been associated with b' as standing in the relation r'', this relation being similar to the one existing in the dispositional knowledge between a'' and b''. The factors favoring substitution will obviously here again be of the same general nature as those given for the other classes.

III

It is clear that such associational conditions as those just discussed may be expected to be of especial importance in carrying out the task of finding the opposite to a given stimulus-word. Many words have, of course, more than one opposite, taking the term in a broad sense. A word may, because of different sides or constituents of its meaning, allow of opposites in different directions, or there may be different degrees of opposition in the same direction. In other cases, again, one word may stand in opposition to another with regard to a side or constituent of the latter's meaning, the two otherwise agreeing to a very large extent. Moreover, if these words are in common use, it may safely be accepted that they have to a very large extent become associated with each other as standing in these different relations of opposition. On account of the relatively subtle differences often existing between the different meanings of opposition, and on account of the fact that there will be differences of total dispositional preparedness for the

[22] Generally expressed, we mean by 'dispositional preparedness' the readiness for response of the memory after-effets to the influence of factors, such as the undertaking of a thought-task tending to bring the contents corresponding to the memory after-effects to consciousness. The memory after-effects include the dispositions for the contents as such, as well as any associations, whether depending on direct temporary contiguity or not, between the dispositions for the contents. Should no such associations be in existence, the total dispositional preparedness becomes identical with the reproductive preparedness of the disposition itself.

different opposites to the same word, we may expect the hypothesis, developed above, as to the rôle which substitution plays in causing errors of thought, to obtain verification by examining the processes of thought involved in finding the opposite to a given stimulus-word. In the present and following sections we attempt a verification of this hypothesis. The data for this part of our discussion consist, in the first place of the introspections of our *S*s, on processes by which they reached solutions unsatisfactory to themselves in attempting to find the 'opposite' to the stimulus words presented.

Procedure. The stimulus-words were given orally. The reaction-times were measured by means of an ordinary stopwatch. If the *S* had not reacted at the end of 30 sec. at most, he was interrupted and asked to proceed at once with as full and accurate an introspective report as possible. Usually, however, the interruption took place sooner. In all other cases the introspective report took place immediately after reaction. For the greater part the introspective reports were registered by the *S* phonographically. In all other cases they were written down by *E* at a speed which would cause a minimum of disturbing delay to the *S*. Few questions were put, and these in a form calculated to make them as little suggestive as possible. Any part of a report forming the reply to a question is quoted in parentheses, remarks by the author inserted in the body of the protocol being put in brackets.

Subjects. Five *S*s took part in the experiments. They were H. F. Verwoerd (V), and J. C. Conradie (C), both of whom had had several years of experience in introspective work; J. Mommsen (M), and P. Stander (S), both of whom had had about one year's experience; and C. Oberholster (O) who had had only a few months training. The *S*s were unaware of the special use to which their reports were to be put.

The experiments were carried out, and the introspective reports given in the mother tongue of the *S*s, namely Afrikaans. Care has, of course, been taken to give the sense of the reports as exactly as possible in translation.

In order to discover the factors which caused the arousal of incorrect solutions, whether overt or not, we must examine the general nature of the processes which took place in arriving at any solution at all, and especially in arriving at a solution which was found satisfactory.

One means by which a solution was reached agrees in all essentials with the process analyzed in the previous section, where one syllable of a series, which had been repeated, is given with the presentation of the task following. For example, x and y are two concepts already known to the *S* as standing in the relation of being opposites. The opposite of x is now asked for, and the answer is obtained as the result of task-directed complex-completion on the basis of the existing dispositional knowledge. The reaction takes place sooner and with fewer intermediary processes according to the freshness and familiarity of this dispositional knowledge. The absence of intermediary processes in such cases means that the complex-completion is of a direct type, and it is only what may be expected when we

find the introspective reports and the reactions of the *S*s, showing a high degree of uniformity in these cases. More for the sake of comparison with the complicated cases to be discussed later on than for the sake of illustrating these somewhat obvious points, we give the set of protocols where the opposite to the stimulus-word "large" was required. The reaction-word and the reaction-time are given after the stimulus-word in each case.

(1) V. Large–small; 1.0 sec. I had the thought in consciousness of giving the opposite, but this idea was not so prominent as in the previous case. I waited for the stimulus-word with a little tension, and as soon as it was given, 'small' immediately came up with the consciousness that it was correct and the opposite of "large."

(2) C. Large–small; 1.4 sec. As soon as I heard the word 'large' I had a visual image of it, and immediately after that the word 'small' came to consciousness as if written after the first. I was conscious that I was quite correct and there was an attendant feeling of pleasantness.

(3) M. Large–small; 1.2 sec. Immediately after the stimulus-word I reacted with 'small.' (I was conscious that I was quite correct.)

(4) S. Large–small; 1.6 sec. When I heard the word 'large' I had a vague sort of recollection of the instructions given just before the stimulus-word was named, and without any processes of which I am now still conscious the word 'small' came up immediately.

(5) O. Large–small; 1.0 sec. The opposite 'small' came up immediately after the experimenter had said 'large.' I cannot recollect that I thought anything further.

In all these and similar cases, the *S* sets to work with the anticipatory schema for the opposite of the stimulus-word and the answer arises immediately on the basis of familiar dispositional knowledge. We wish now to give a series of examples of protocols of another type of frequent occurrence and then to proceed with a discussion of them.

(6) S. Nothing–everything; 3.2 sec. As soon as I heard 'nothing' I at once attempted to fix its meaning and then I tried to carry out the task. The word 'much'[24] came up first of all. I noted however that this was not the correct word for the meaning of the opposite. Then the idea occurred of "everything that exists" and I reacted with 'everything,' strongly conscious that it was correct.

(7) M. Nothing–something; 5.8 sec. When I heard 'nothing,' I at once thought of Hegel's nothing and then I recalled the task to consciousness. Then the word 'much' came up, but I was at once aware that it was not suitable as the opposite of 'nothing.' Then the word 'something' arose for the first time, and after this idea had occurred to me, I had the feeling that I was now almost quite correct. While I was introspecting, 'everything' came up in consciousness and I at once saw that this was a much more suitable word than 'something.'

(8) C. Nothing–much; 3.2 sec. After 'nothing' had been given 'much' immediately came up in my consciousness, but before reacting I thought whether it was absolutely the opposite of 'nothing.' I reacted after it appeared to me that there was not another word for me personally which I could place over against 'nothing' as its opposite.

[24]The Afrikaans word used was *baie* which means 'much' or 'many.'

(9) C. Nothing–something; 1.8 sec. When the word 'nothing' was given I had a vague idea of a hat without a brim of which the crown was gone. I felt half inclined to laugh, but at once thought of my task of giving a word with the opposite meaning. A kind of image (I cannot quite say that it was one) of the word 'something' came to consciousness and at the same time I had a clear image of the word 'nothing' as written. (I had a strong feeling at the time of reaction that my answer was correct.)

(10) V. Nothing–something; 1.6 sec. I sat and waited for the word with the idea that it would again be one which would at once cause difficulty. When the word 'nothing' came, I did not think of 'opposite,' but concentrated altogether on 'nothing.' In connection with the stimulus-word there was a very vague impression (I cannot say whether it was in the form of an image or not) of something which is erased and in connection with this last again I reacted with 'something' before I had tested and without any impression whether it was correct or wrong. But just after I had reacted I had the idea that it must be correct, because something is erased so that the one must be the opposite of the other. I did not now think of the *word* 'opposite.' I thought of the *idea* by means of 'what lies in another direction.'

(11) O. Embarrassed–[no reaction; interrupted after 8 sec.] Immediately after the experimenter had given the stimulus-word, I had a visual image of a person who is embarrassed—somebody standing with his head bent forward. Then I thought what the opposite of 'embarrassed' is. After that the word 'cheerful' came up, but this did not satisfy me as the opposite of 'embarrassed.' The word 'joyful' also came up but this also did not satisfy me.

(12) C. Love of ostentation [Afr. *praalsug*]–humility; 10.6 sec. I was again put out when I heard the stimulus-word. It was as if I had an impossible case before me for which I would not be able to get the opposite. First of all I thought I might perhaps answer 'humility' but the answer was not to my mind a satisfactory one. I then again concentrated on the stimulus-word but could, so to speak, get no further. Finally I answered 'humility' with the consciousness that it was incorrect.

(13) V. Desirous of honor [Afr. *eergierig*]–without ambition; 12.6 sec. I did not note at all, this time, that the experimenter said 'opposite of,' and had no idea—or at any rate no clear one—in consciousness before the stimulus-word came of the meaning of 'opposite.' When the stimulus-word was given, it seemed to me that it would be difficult to give the opposite. At the time I was not thinking clearly that what must be given is the opposite. After the stimulus-word had been given I thought of 'humble' and submissive,' but these were not satisfactory, because they did not deny that desire or urge for honor which was implied by the stimulus-word. I searched for the opposite of this word for a time. This searching consisted of the stimulus-word coming to consciousness every now and again, and of my attempting to grasp its meaning. I thought of the meaning formulated in words, namely, desire for honor. Further, the searching consisted of trying to get another word which would be the opposite. The idea of the opposite meant for me 'without desire for honor.' I was not able to get any single word for this. The idea of ambition came to consciousness while I was going through the process of searching, just described. I became conscious, however, at one point that 'without ambition' would fulfil the requirements of my task. I was not thinking of opposite, however, at the time but rather that the words 'without ambition' meant what I was thinking I must also get 'without desire for honor.' There was a vague idea, while I was searching, of people who are without any hope in life and a vague image of a person who is bent and hopeless. When I was thinking about 'without ambition,' I was not so conscious of my desire to get a single word with that meaning. After I had reacted with

'without ambition,' I noticed that I had not got a single word and that I could rather have given 'unambitious.' Before I reacted, however—I should say by way of testing my answer—the stimulus-word came fleetingly to consciousness with the idea of seeking the opposite to it, as if the stimulus-word were colored with this idea, and this double idea was put into relation with 'without ambition.' This satisfied me.

The majority of the examples (6) to (13) are cases where the *S*s were not satisfied with solutions which occurred to them. Examples (8), (9) and (10), in which dissatisfaction was not so definite, are adduced for purposes of comparison. The first question we wish to consider is: By what psychological process or processes do solutions impress the *S* as not being acceptable or not fully acceptable? Several different types of cases have to be distinguished, of which one especially interests us here. It is characterized negatively by the fact that the task was not misunderstood, the solution being, on the contrary, anticipated in accordance with the task actually set. The discovery of the error does not depend on the discovery of a discrepancy between the two tasks. It is, furthermore, characterized negatively by the fact that no special process of testing, either resulting from a decision to test after the solution had offered itself or occurring without such decision, intervenes between the occurrence of the solution and the first consciousness of its inadequacy. This type of case is characterized positively by the fact that a solution offers itself which does not correspond with the anticipation in some respect, and that this non-correspondence comes to consciousness in accordance with the law of conflict. This law, accepted by us, runs as follows: If one conscious event takes place instead of another which has been expected (or anticipated), then the first as well as the point of conflict or difference, tends to be noted without especial direction of attention towards them.[25]

If, now, we have an anticipation agreeing with the task set as far as content is concerned, but so constituted in consciousness that one or more of the constituents of the anticipation do not have sufficient directive efficiency, then this constituent may still be present to a degree sufficient to bring about the recognition of the relation of discrepancy between the result arrived at and the one anticipated. Thus the negative judgment, with regard to the correspondence between the anticipation and the solution offered, becomes an indirect criterion for the presence of the anticipation of the corresponding positive relational content (*Sachverhältnis*) in these cases of task-directed processes of thought.[26]

We hold that some of the protocols from (6) to (13) are clear cases falling under the law of conflict. In (6) the solution

[25] Selz's *Gesetz der Widerstreitsbeachtung*, I, 275; and II, 217.
[26] Selz, I, 270; and II, 217.

'much' is followed by the judgment that it is not correct as the opposite of 'nothing.' In (7) the *S* states that this awareness came at once after the solution 'much' had occurred. In (11) the occurrence of the solutions 'cheerful' and 'joyful' as the opposite of 'embarrassed' is followed by the consciousness of their not being satisfactory, and we find a similar state of affairs in (12) with regard to 'humility' as the opposite of 'love of ostentation,' except that the answer perhaps made the impression of being a doubtfully possible one before being looked upon more definitely as incorrect. In (13), too, the first pair of solutions ('humility' and 'submissive' as opposites to 'desirous of honor') are followed by the consciousness that they were not suitable as not having that characteristic for the *S* which attached to the opposite which he was seeking. Applying Selz's indirect criterion, we thus come to the conclusion that in these cases the solutions arrived at, but found unsatisfactory, did not correspond in some respects, at least, with what was actually anticipated.[27]

It is even possible to indicate in some cases the direction which the anticipation was taking before the rejected solution occurred. In order to show that this is the case we must explain certain characteristics often present in our protocols and exemplified in some of the cases adduced by us. According to the law of conflict, a point of discrepancy between an anticipation and a solution offering itself tends to be noted. This solution must then have occurred notwithstanding the fact that there was actually present in the anticipation a constituent in which it differed from the solution. This constituent of the anticipation was however not present clearly enough in consciousness, and so not efficient enough, to prevent the noncorresponding solution from arising. The rejection of the solution according to the law of conflict implies that this constituent must now have attained to some degree of attention, although not necessarily in a clearly analyzed form. This in its turn means that the anticipatory conditions have now been improved for attaining a solution which corresponds better with the original anticipation, since that part of it which was not at first present efficiently enough has now been brought to clearer consciousness. Hence the conditions are more favorable now for the occurrence of a fresh solution corresponding to the first in so far as the new anticipation agrees with the original antici-

[27]It may be pointed out that, on our view, the law of conflict does not imply that the point of discrepancy between anticipation and solution arrived at should be so clear to the *S* that he is able to formulate it clearly. In view of the subtle differences in meaning often existing between different opposites to the same word a considerable amount of thought specially directed to deciding the point of difference is often necessary before it can be brought clearly enough to consciouaness to be formulated. One would then be carrying out a process of conceptual analysis quite different from the processes taking place in the *S*'s mind in such experiments such as ours.

pation, but differing from the first solution with regard to the point of discrepancy, if this latter attracted sufficiently clear attention at the moment of rejection.

In this way we can explain one important type of case common in daily life, where a rejected solution, so far from having merely played the rôle of a hindrance, has also fulfilled the function of assisting us to get to the correct one. We formulate our conclusion by saying that, *if an improvement of an anticipation takes place, in one or other respect as a consequence of the rejection of a solution which occurs, and if another solution offers itself as a result, then the second solution provides a further indication, which is absent from the first solution, as to what the direction of the original anticipation was.*

Protocol (6) shows the characteristics required by the view just developed. The solution first occurring in trying to find the opposite of 'nothing' was 'much.' After its rejection the solution, with which *S* eventually reacted as satisfactory, occurred, without an intermediate process being mentioned, in the form of "everything that exists." In protocol (7) the first solution for the opposite of 'nothing' was 'much,' and after this had been rejected the solution 'something' offered itself, without an intermediary process being mentioned, as being almost quite correct. The process by which a third solution even more satisfactory to the *S* was obtained, in this case shows a peculiarity to the discussion of which we must return. The following protocol is another case in point.

(14) M. Lazy–industrious; 2.0 sec. Immediately after I had heard the stimulus-word I had the feeling that the task was easier than in previous cases. There was a pleasant feeling, of medium intensity, also present. Then the word 'industrious' came up, and I at once had the feeling that this was the correct word with which to react. Before this, however, I had thought of 'flink,'[28] but I was at once conscious that this was not the opposite of 'lazy.'

The *S* here again, as in the cases of (6) and (7), first of all came on a solution lying in the same direction as the one eventually accepted, the comparative shortness of the reaction-time also showing that the second solution must have occurred very quickly after the rejection of the first. In (8), (9) and (10) no second solution offers itself. In the case of (11), two solutions, both of which are rejected, are mentioned, without the *S* stating that he found one better than the other. Even if he actually did not, this fact would not contradict the view developed above as to the function of the process of rejection. The original anticipation may be especially poor, so that a point of discrepancy does not come to consciousness clearly enough to

[28]This Afrikaans word, which means approximately 'active' or 'smart,' agrees partially n sound and spelling with the reaction-word *fluks* (industrious).

cause a noticeable improvement in the consequent solution. We shall note later a special reason for believing that this was the case here. In the case of (12) the solution first occurring was indeed found unsatisfactory, but the *S* was unable to find a better one. Since the finding of a solution which is satisfactory to the *S* clearly does not depend only on the anticipatory conditions, this protocol also is not inconsistent with the view developed above as to the function of rejection. In the case of (13), the closely connected solutions "humble" and "submissive" were rejected as not fulfilling the requirements of a satisfactory solution in a particular respect. A further process of searching then took place in which this characteristic was a prominent part of the anticipation. This is in clear agreement with our view of the function of rejection.

We have now reached the stage at which we can put our second and fundamental question: Why is it that a solution can occur which shows a discrepancy with the direction of the anticipation? Several of the protocols adduced provide indications in support of the view that a certain amount of task-displacement in Selz's sense took place. Thus, it may be urged that, in the case of (9), a distraction of attention from the task, leading to task displacement, was produced by the tendency to laugh and the contents causing this tendency. In the case of (11), previously referred to in connection with this point, the indication is especially clear. The *S* thought the meaning of the stimulus-word 'embarrassed,' at least in part, by means of an image of a person with head bent forward. This is an attitude characteristic of sadness or sorrow, and in agreement with this we find that "cheerful" and "joyful" now offer themselves when the *S* tries to carry out the task. In the case of (12) the *S* states that he was "put out." In the case of (13), the *S* reports that there was at least a lack of clearness, with regard to the meaning of 'opposite,' before the first, unsatisfactory, solutions occurred. In the case of (14), again, the *S* mentions a pleasant feeling of medium intensity as having arisen along with the impression that the task just given was an easy one. In addition, it may be said that particular characteristics as to the kind of opposite wanted were not present clearly enough in the anticipation, but that the abstract characteristics common to the different meanings of the word 'opposite' were unduly prominent. We thus have present a number of those factors which Selz advances as leading to task-displacement and partial efficiency of the task set. In (13), furthermore, there is an example of a type of occurrence, also present in other protocols than those adduced, namely, where a mistake is discovered, or a better solution offers itself, at, or just after, the moment of overt reaction. In this case the *S* finally gave "without ambi-

tion" as the opposite of the stimulus-word, and the protocol explicitly states that a particular characteristic of the solution sought, viz., that it should be a single word, had by now been in a measure lost sight of, although it had evidently been present in a higher degree at an earlier stage. The fact that the overt reaction is, as it seems to be, favorable for the discovery of a mistake and a subsequent improvement, is, we hold, due to a special concentration on the part of *S*, previous to reaction, on certain characteristics of the solution to be found, with the result that other characteristics are neglected. When this concentration relaxes at overt reaction, the neglected characteristics can more readily make their presence felt, *i.e.* the discrepancy may now be discovered according to the law of conflict. Possibly a somewhat similar factor may also be partly responsible for the belated "everything" as the opposite of 'nothing' in the case of (7). Still another answer is suggested by protocol (6). Here there are no positive indications, of the kind adduced above, that the *S*'s attention was distracted from the task. But it may be argued that there was a lack of clearness in his mind as to what was really required and that, consequently, a shifting of the task in the direction of a related one took place. Selz himself holds that the early discovery of an error, as was the case in (6), is an indication that a task-displacement of the above nature has occurred.[29] This, however, exhausts the list of cases adduced up to now where solutions occurred which were rejected or definitely found unsatisfactory by the *S* himself. The conclusion might, therefore, be drawn that the explanation of the errors is to be found among the anticipatory conditions.

As against this it must now be emphasized, according to the conclusions reached previously, that even admitting that the anticipations were not in all respects clear, still the anticipatory conditions themselves were more favorable for solutions other than those which occurred first of all and were rejected. Beside the anticipatory factors, there must, therefore, have been another factor, or other factors, present which favored the early occurrence of solutions appearing unsatisfactory to the subject according to the law of conflict. These must, moreover, have been present to such an extent as to more than compensate for relative disadvantage accruing to the rejected solution from the nature of the anticipation.

According to the analysis given in Section II we may expect such compensating factors to be present in the form of a high dispositional preparedness of the rejected solution. This expectation, following deductively from general principles, is

[29]Selz, II, 18.

corroborated by the examination of the cases adduced. In the case of (6) the solution first of all offering itself but rejected was "much." This was also the case in (7), and in (8) the *S* actually reacted with this word as the opposite of "nothing," though there are distinct indications in the protocol that he, too, did not find it completely satisfactory. In 3 of the 5 cases where our *S*s were given this stimulus-word we find the same error occurring. The two other protocols with the same stimulus-word show a similar uniformity, for in both (9) and (10) we find the *S*s reacting with "something." No process of previous rejection took place and the reaction-times are very short, corresponding closely to those of protocols (1)–(5), where the answer found satisfactory was arrived at by means of direct complex-completion on the basis of familiar dispositional knowledge. In addition, protocols (6)–(10) show essential agreement on this point, up to the moment where the first solution offered itself, with what is reported in (1)–(5), that is, this solution occurred very soon after the task as a whole had been undertaken. Certain kinds of opposition are evidently in concrete cases oftener used in daily life than others. This is clearly so with the pairs 'nothing–much' and 'nothing–something' as compared with 'nothing–everything.'[30] The fact that in the case of (10) the reaction took place before consciousness of the correctness or incorrectness of the solution had developed, is itself typical of a reaction occurring as the result of strong associations, since reactions then tend to become automatic. In the case of (12) the *S* reacted with the first solution occurring to him, namely "humility," although he did not find it satisfactory. Two of the other *S*s (M andO) also happened on the same, or essentially the same, solution as the opposite of the stimulus-word concerned.

(15) M. Love of ostentation (*praalsug*)–humility (*needrigheid*); 12.6 sec. After I had heard the stimulus-word I thought of ostentation (*praal*) and magnificence (*prag*) and of the well-known phrase in which these words occur. I then thought of the task given me, and then tried to react as quickly as possible. First of all the word 'modest' occurred and then the word 'humility,' with which I reacted with a vague idea that it might perhaps be correct.

(16) O. Love of ostentation–[no reaction; the *S* was interrupted after 15 sec.] I sought for the opposite of the word. Then a word like 'simple' came up first of all, but this did not satisfy me as the opposite of the stimulus-word. Then a word like 'humble' occurred which also did not satisfy me as the opposite of the stimulus-word. I then began thinking which of the two, 'simple' or 'humble,' would be best suited as opposite but I could not come to a decision.

[30] The Afrikaans words *niks–alles* (nothing–everything) have not the similarity in sound and spelling which the English equivalents have and which would probably aid the reproduction of 'everything' by 'nothing.' In addition, the Afrikaans *baie* ('much' or 'many') because of its two meanings, both of which stand in a relation of opposition to 'nothing,' would for this reason be used more frequently as the opposite of *niks* than 'much' would be used as the opposite of 'nothing' in English.

Thus, in three of the five cases the *S*s happened upon essentially the same solution for the given stimulus-word. As in the cases of protocols (1)–(5) and (6)–(10), this uniformity is indicative of a relatively high state of reproductive, or dispositional preparedness of the solution, resulting from the concepts being used frequently in daily life. The *S*s, V and S, did not show a similar uniformity with the other *S*s in reacting to 'love of ostentation.' Similar preparedness is, however, indicated for V by protocol (13) where 'humble' formed the first rejected solution on another stimulus-word but related in meaning to 'love of ostentation;' furthermore, C also reacted with 'humble' on this last stimulus-word, the protocol giving, in addition, further evidence of the rôle which such preparedness plays with regard to the solution "modest," which occurred first of all but was rejected.

(17) C. Love of honor–humble; 13.2 sec. When the stimulus-word was given, I was at once put out altogether by the abstract conception. I then tried to think of the [*immediately*] preceding case of 'modesty' and its opposite, and I wanted to react at once with 'modest,' since it seemed to me that a modest person is one who is the opposite of one who is desirous of honor. I thought, however, that it did not seem to be a sufficient opposite and so I did not wish to answer at once. I waited quite a little time in a fairly unpleasant state of mind because it seemed to me that I must answer 'humble,' and again there was a moment of doubt. I felt that my answer was wrong and I gave it being quite uncertain with regard to its correctness or incorrectness.

It is true that in protocol (14) the solution which first occurred and then was rejected by the *S*, does not present itself in any of the other cases with the same stimulus-word, and in the case of protocol (11) there is a similar lack of uniformity with the solutions of the other *S*s, the protocol showing in addition clear indications of task-displacement as the result of poor anticipatory conditions. As in the cases already discussed this latter fact does not fully explain the arousal of the solutions rejected by *S*, and the absence of uniformity in reaction as an indication of the reproductive or dispositional preparedness of the answer does not necessarily imply the absence of such preparedness, since there are, naturally, individual differences with regard to the latter.

In some cases there are additional indications that the *S* had no satisfactory answer readily at his disposal, the result of the latter fact being, again, favorable for the occurrence of solutions running contrary to the anticipation. Such indications are the impression of difficulty which the *S* mentions, the length of the reaction-times and the fact that a special method is made use of by *S*.

This method contains the following as essential steps: The stimulus-word or concept(A), or one or other of the character-

istics (B) of A, is first of all negatived, so that the *S* attains, *e.g.* to non-B. He then proceeds to search for an expression of a more positive nature or meaning (C) which contains non-B as constituent, since a *mere denial* is often not considered to be a satisfactory solution. If, now, non-B forms part of a previously existing dispositional knowledge of C, then C may itself be brought to consciousness on the basis of this knowledge. The recognition of a relation of opposition between A and C may now follow.

The following protocol is adduced as an illustration of this type of case.

(18) V. Love of ostentation (*praalsug*)–slovenly; 6.6 sec. There was a tendency present with me now to look for a word in the direction of not loving ostentation. This arose in conjunction with a very vague consciousness of opposite. When I had got to the idea of not loving ostentation, I was however no longer thinking of opposite but I was trying rather to get a word which meant that idea precisely. This searching consisted of keeping the meaning 'not loving ostentation' continually in consciousness and of waiting for another word which included this meaning to come up. While I was busy with this process of searching, the idea suddenly came up that it was not absolutely the opposite if some one merely did not love ostentation, but that he must also be dirty. I had this idea of dirty—which I think was imageless—in the form of thinking of a dirty person with slovenly clothes. There was no word in my consciousness but just this impression of dirty. I had this idea in consciousness for a short time, and then I had the impression of words like 'untidy,' 'slovenly,' or 'dirty.' The words did not come clearly to consciousness but there was just the impression that I knew a word in that direction. I can not describe it exactly. In conjunction with this consciousness the word 'slovenly' came up suddenly. I compared 'slovenly' with the stimulus-word by thinking of the two as far as possible at the same time and I thought of contrast and opposite. I saw that the contrast consists of the fact that the one includes everything that the other one does not include. Then I was satisfied.

The following protocols are further examples of this procedure, but the solutions run contrary in some respect to the anticipation directed towards obtaining a more positive result after the negation has been reached.

(19) M. Answer–[no reaction; interrupted after 20 sec.] As soon as I heard the stimulus-word, I saw that it would not be easy to get a suitable opposite in this case. I then had word-images of 'no answer' and of 'keeping silent' [*swyg*]. I was then conscious that 'answer' was a noun and 'keeping silent' a verb, and that it would thus not be possible to give the latter as opposite to 'answer.' Then the word 'silent' occurred, but it was at once clear to me that 'silent' would not be the correct word with which to react. Then I made vain attempts to get a suitable opposite at once. . . .

(20) V. Reason–reasonless; 5.0 sec. Immediately after the stimulus-word had been given I thought of 'unreason' [*onrede*], but this did not satisfy me, because it was, for me, a mere denial of 'reason.' I then thought that 'reason' could be taken in the sense of 'cause' and I then sought for a word for 'without cause.' I could not get such a word at once. Then I thought of 'reasonless' [*redeloos*]—I think in connection with the idea mentioned above of 'without reason,' though I am not certain. When

'reasonless' came to consciousness I was in doubt for a moment because it did not stand for the idea I wanted to give. It meant something in the direction of a person who is mad, whereas I wanted 'without reason' [in another sense]. I then thought, however, that 'reasonless' could be taken in two senses and that I could use the word as I wanted. There was just the idea present in passing that 'reasonless' was the denial of (but also meant the opposite *class* to) 'reason' and was in so far the opposite.

In these cases, then, we find present, first, the process of negation. In (19) the *S* attains to the negation "no answer" in seeking for the opposite of "answer" and in (20) to what is for the *S* a mere denial, namely "unreason," or "without reason." Then, in (19), in connection with "no answer" the *S* reaches "keeping silent" and "silent." In (20) "reasonless" offers itself. It has indeed, also a negative form but also, for *S*, a further positive meaning not agreeing with the anticipation. We may accept that 'reasonless' with the positive meaning attached did occur in connection with the negation 'without reason,' although the *S* was unable to recollect in giving his introspective report that such was certainly the case. In both (19) and (20) the recognition of the more positive form or content as not agreeing with what was to be obtained, evidently takes place according to the law of conflict.

As in previous cases, we may, therefore, regard protocols (19) and (20) as providing examples of the type of case where a satisfactory answer corresponding to the task and anticipation is either (a) not present at all in the form of dispositional knowledge, or (b) not present in a state of preparedness sufficiently high to prevent an unsatisfactory answer occurring.

Even granting that the anticipations were lacking in clearness in at least some respects, obviously the occurrence of non-corresponding solutions cannot be explained by this fact alone. Just as the solutions anticipated will be reached, as Selz points out, if the dispositional knowledge of them is sufficiently fresh and firm, even though the anticipatory conditions do lack clearness and prominence in all respects, so, too, solutions not corresponding to the anticipation will present themselves, if that part of the dispositional knowledge of them which does not correspond to the anticipation is sufficiently fresh or firm. These constituents of the dispositional knowledge will then act in the direction of *overcoming* the influence of the anticipatory conditions *already realized* in so far as these latter tend to act in another direction. We have thus to distinguish this type of factor from disturbances which weaken the anticipation itself *initially* and are thus also favorable to the occurrence of solutions not corresponding to the anticipation in all respects.[31]

[31]Cf. Selz's statement that "Abweichungen der intellektuellen Prozesse von der schematischen Antizipation ihres Erfolgs können trotz ihrer richtunggebenden Funktion allerdings dann entstehen, wenn einzelne Bestandstücke der Antizipation, z. B. infolge von Störungsreizen, unwirksam bleiben. . . ." O. Selz, *Die Gesetze der Produktiven und Reproduktiven Geistestätigkeit*, 1924, 14-15.

We thus, reach the conclusion that the data advanced in this Section demand the assumption of the validity of the law of substitution in order to be satisfactorily explained. This result is in full agreement with that already obtained in Section II. We have, however, not yet examined Selz's objections against the law as stating a causal condition for the occurrence of errors in thinking. This will form the subject-matter of the following section.[32]

IV

As indicated in the first section, Selz also found cases in his experiments where solutions occurred which did not correspond to the anticipation. He holds, however, that this was due to the fact that the anticipation was not fully efficient, and maintains that his observations thus justify the hypothesis that in the case of task-directed processes at least, the so-called law of substitution is not valid for the relation of anticipation and result obtained, whether this result be of a reproductive or abstractive nature; or, should the law have validity, that it holds within such narrow limits of similarity that the differences of anticipation do not in general act as sources of error. If the anticipation is fully efficient, according to Selz, only those processes of reproduction (and abstraction) occur which correspond to the anticipation, and those which would correspond to a similar but partly deviating anticipation are absent. If, by way of exception, a result does occur which does not completely correspond to the anticipation, then this result is due to the fact that those constituents of the anticipation to which the result fails to correspond, had a lower degree of efficiency than those to which the result does correspond. For example, according to the law of part-efficiency, this might be occasioned by a reduced degree of consciousness or of dispositional efficiency, so that the anticipation acted as if the constituents to which the result does not correspond had not been present at all.[33]

The expression, "full efficiency or activity of the anticipation," may be taken in two senses, and, unless these are kept clearly apart, it might well appear that the law of substitution plays no part in the causation of errors of thought.

In the first sense, it may be used merely to indicate the *fact* that complete correspondence between anticipation and result

[32]We need not discuss an objection which Selz raises (II, 376) against the law of substitution as an explanation of *another* phenomenon, viz., the fact that, if given tasks having the same general characteristics, we make use of operations of the same general nature. Even granting, as we do, that this objection is well founded, we may still consider the law a valid explanation for the type of case here under discussion.

[33]Selz, II, 409 f.

exists. Where this is so, substitutional processes have by hypothesis, of course, played no rôle. This is naturally no argument against the validity of the law of substitution for the case of processes consequent on the undertaking of a task, unless it can be proved—which, of course, it cannot be—that all anticipations are fully efficient in the sense just given.

The second sense in which the expression "full efficiency of the task" can be taken is an *explanatory* one, namely, that all the constituents of the task were so clearly and strongly present, and thus so causally active, that a solution not corresponding to the anticipation in all respects was actually prevented from coming to consciousness and that a solution having this correspondence did occur.

It is clear that the sphere within which processes taking place according to the law of substitution can act will decrease, other things being equal, with increasing strength and clearness of the constituents of the anticipation. On the other hand, however, a decrease of clearness or strength of one or another anticipatory constituent will increase the sphere within which the law of substitution may act.

A similar ambiguity attaches to the expression "part-efficiency of the anticipation." It may, first, be used merely to indicate the *fact* of non-correspondence between anticipation and result. *All* cases of the result showing such non-correspondence are then cases of part-efficiency of the anticipation. But part-efficiency in this sense is then merely a *name* for the phenomenon of non-correspondence, just as full-efficiency in the first sense is then merely a *name* for the phenomenon of correspondence between anticipation and result. But it is just these phenomena of non-correspondence which have to be explained, and the law of substitution is an explanation offered. Part-efficiency in this sense and substitution are then not rival explanations. The acceptance of the one does not imply the non-acceptance of the other. We may, secondly, take part-efficiency in an explanatory sense. It would then mean that there are constituents of the anticipation which are not clearly and strongly enough present to prevent solutions not corresponding to the anticipation in all respects from coming to consciousness. For the latter case, again, we see that the sphere within which the law of substitution may act will vary inversely with the strength and clearness of the constituents of the anticipation. In short, if we take the expressions "full efficiency" and "part efficiency" as explanatory, and not merely as names for correspondence or non-correspondence between anticipation and result, we are not justified in setting

the law of part-efficiency over against the law of substitution in such a way that the acceptance of the one implies the rejection of the other, or in holding that because part-efficiency of the anticipation can be shown to have been present, therefore substitutional processes are excluded.

Selz, furthermore, states that it is incomprehensible under the law of substitution why the idea a″, being similar to a′, should arouse associations connected with a′ only in the case of distracted attention, and not also in the case of full attention, and advances this as an argument against the law of substitution and in favor of an explanation by means of his law of part-efficiency.[34] We may reply that distraction of attention provides just those conditions of part-efficiency which are favorable for the occurrence of processes in accordance with the law of substitution.

How, then, does Selz explain the experimental results of Müller and Pilzecker which were presented in Section II? He holds that if a syllable has largely been forgotten, so that only those parts to which special attention has been given can be reproduced, then if another syllable should present itself which corresponds completely with this part, it may be looked upon by the subject as probably correct. It is, therefore, not the similarity of the two syllables which makes the substitution possible, but the complete correspondence with the correct one so far as it is still remembered. So far Selz's view is certainly to be accepted. We have ourselves followed it in the discussion in Section II. Such acceptance does not, however, prevent acceptance of the law of substitution, since it is clear that the valid portion of Selz's argument really explains only why errors arising *according to the law of substitution* are sometimes not recognized as errors. It is, furthermore, clear that the complete correspondence to which Selz refers, will occur, other things being equal, with a probability which increases as the degree of similarity between the syllables increases.

Another argument of Selz's against the validity of the law of substitution as an explanation of Müller's and Pilzecker's results appears on further examination to be an argument in its favor. He points out that the series of syllables were not thoroughly learned and holds that, if errors should arise at testing in this case according to the law of substitution, then such errors should also occur with a series which has been thoroughly learned, if the substituted syllable lacks strong associations of its own. The fact that such errors increased with lapse of time is viewed by him as pointing rather to the supposition that

[34] Selz, II, 555.

the cases of substitution occurring are not processes falling under the law of substitution but are due to the result of weakness of the memory after-effects (*Reproduktionsgrundlagen*).[35] As against this argument of Selz's, it must be pointed out that the weakness of the dispositional knowledge of two syllables which have been associated as standing in a certain relation, is merely a factor favoring the occurrence of mistakes according to the law of substitution. Weakness of the dispositional knowledge by means of which the correct answer would be obtained is not a rival explanation to that by means of the law of substitution, but is, on the contrary, a part of the latter.

Finally, Selz holds that, if the law of substitution were valid for a large sphere of similarity—and the examples of Müller and Pilzecker show similarity of only one letter—then we should expect mistakes to occur more frequently, *e.g.* when the *S* is required to give a rhyme to a nonsense-syllable.[36] We may reply that there are special conditions present in such a task as rhyming which prevent an incorrect reaction from being looked upon as correct, and that these conditions are not present in the same degree in the case of the reproduction of syllables, especially if these have been insufficiently or not recently repeated. In rhyming we have, in the stimulus-syllable, a content actually present or easily recalled, on the basis of which the incorrectness of a reaction, resulting from poor rhyming mechanism, may be recognized at once. In the reproduction of syllables such knowledge, by means of which the incorrectness can be recognized, is very often absent. In addition to this it appears to us that in rhyming the anticipation will on the whole be fuller than in the case of reproducing a syllable from a series which has been learned. The syllable which has to be matched with a rhyme is actually present at the stage of anticipation, so that the characteristics of the reaction to be made are comparatively fully given. This fact would also materially increase the chances against the occurrence, overtly or otherwise, of an incorrect reaction.

Our arguments in favor of the validity and sphere of application of the law of substitution do not, however, imply that processes taking place according to this law occur in all cases of errors of thinking. The type of case which we have examined and in which such processes play a rôle is characterized by the fact that *one solution is anticipated but that another offers itself on the basis of existing dispositional knowledge.* Errors do, however, also occur where the anticipation is at fault, either in the

[35] Selz, II, 411 ff.
[36] Selz, II, 410. Cf. the discussion in Section II.

sense that some constituent of the problem whose solution should have been attempted was not present in the anticipation at all, or in the sense that additions were unwittingly made to this problem. In such cases substitutional errors, taking place after the anticipation has been formed, are not necessarily involved in the causation of the error.

The individual cases adduced by us were characterized by the fact that the *Vs* became aware of the non-correspondence between the anticipation and the result arrived at. However, not all thought-processes in which substitution takes place, need have this characteristic. Constituents of the anticipation may, indeed, conceivably have been present in consciousness, but, for example so slightly that they were not able to make their presence felt sufficiently for the non-correspondence between anticipation and solution to come to consciousness. In such cases processes of substitution would assist in bringing about undetected errors in thinking.

If there are dispositional factors exerting an influence running contrary to the direction of a constituent of an anticipation actually present, then the relative strength of these two kinds of factors will determine whether or not a result corresponding in all respects to the anticipation is reached. Competition between various directions of complex-completion is not limited, as Selz holds,[37] to those cases where each of the different completions corresponds to the schematic anticipation, but occurs also between directions of completion where the one does fully correspond to the anticipation and where the other does not do so. If one adduces, as Selz does, familiarity (*Geläufigkeit*) of the non-corresponding result as a condition making for errors, one implies the truth of the statement just made.

The examination of the meaning of the laws of part-efficiency and of substitution, of the data advanced in this essay, and of Selz's own arguments against the second of these laws, has led to the same result. Substitution and full efficiency of the anticipation are indeed opposed; not, however, as conflicting theories, but merely as causal factors, working in different directions. Part-efficiency of the anticipation, on the other hand, does not limit, but rather enlarges, the sphere of the processes taking place according to the law of substitution.

Summary

(1) The problem treated is that of errors occurring in task-directed processes of thought, the particular point at issue being

[37] "Entsprechen dem Komplexschema mehrere Komplexe, so findet allerdings eine Konkurrenz mehrerer Prozesse der Komplexergänzung statt. Diese Konkurrenz bewegt sich aber in dem engen Bereich der durch die schematische Antizipation festgelegten aufgabegemässen Reproduktionen. . . . ." O. Selz, *Die Gesetze der Produktiven und Reproduktiven Geistestätigkeit*, 13.

Selz's denial that processes take place in such cases according to the law of subsitution.

(2) After a statement of Selz's own theory and an analysis of the meaning of the law of substitution, experimentally obtained introspective data are advanced to prove that the validity of the law of substitution must be assumed in order to explain errors of thinking arising in a certain type of task-directed cognitive processes. This type of case is characterized by the fact that solutions occur for which the anticipatory conditions are relatively unfavorable since they do not agree with the anticipation. As against Selz it is shown that such facts make it necessary to assume the validity of the law of substitution as a basis of explanation, supplementary to that advanced by him, namely, partial efficiency of the *Aufgabe*.

(3) A number of arguments advanced by Selz against the law of substitution are then analyzed with a result in full agreement with that stated in the previous paragraph.

ATTENTION AND CLEARNESS IN THE PERCEPTION OF FIGURE AND GROUND[1]

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Perception and Attention

In the course of the numerous attempts in the history of psychology to weave the ever-growing masses of data into a comprehensive whole there have arisen many schools or systems; it is significant that these schools or systems have been measured very largely by virtue of their treatment of one problem, the problem of perception. Perception has presented, in this long course of system-building, a dire, entangling difficulty, a difficulty that is grounded in the general fact that whenever, according most strictly to the established principles of a system, there is wrought a description of a perceptual experience, the disturbing conviction nevertheless remains that something still is lacking, that a 'something' was present in the phenomenon that has not yet been caught up into the descriptive account. The conviction of this incompleteness of the perceptual record has led over, characteristically, to a theory of attention, or of something closely allied to attention, as some factor that 'selects,' or 'discriminates,' or 'points out' these particular aspects of the total environment for occupation of central place in the experience, and which shapes, imbues, or colors the experience in some manner.

As a result of this situation it has come almost to be the rule in traditional psychological systems to call frequently upon some theory of attention. Whenever, in the face of a demand for adequacy to a complete description, the perceptual account has found itself in sore straits, then the services of the attentional concept have been summoned. At least, such has been the claim, and the loudest bewailers of this condition of affairs go farther in maintaining—with more than a modicum of justice—that in the theory of attention has too readily been found the surcease of systematic sorrows, that the satisfaction gained from the completion of a description by invoking the attentional principle is too often premature and without substance. Such challenging of the use and validity of the concept of attention has proceeded in most emphatic formulation from the school of *Gestaltpsychologie*, a school that has found its beginnings in the theory of perception, and proposes to get along here, and in the remainder of its system as well, without recourse to any attentional theory, and proposes further—since it has no use for it, and perceives a danger in it—that attention with all its various appurtenances be unreservedly cast away.

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Although it is plain that the theory of attention which the *Gestalt*-psychologists have been concerned about is one particular theory, namely, that which has known its growth in the Wundtian tradition, yet the spokesmen of their doctrine have taken no care to deal discriminatingly in the matter, but have condemned the attentional concept in a fashion that is nothing if not whole-hearted. But one might very well take a little time to this question of attention as it relates to perceptual theory before placing a comprehensive taboo upon it, and seek to demonstrate cogent reason before condemning uncompromisingly all possible conceptions concerning it. This is a large order, of course, for theories of attention have been many. Since one must be compelled to limit the field, let there be selected for consideration in respect to perceptual theory a concept of attention that was framed in cognizant view of the difficulties and dangers in the field, a concept which sought to be based, not upon some hypothetical factor outside of experience, but upon discoverable aspects of experience itself. Such is the theory developed and defended by Titchener, and characterized by the contention that the clearness of the perceptual contents is the fundamental mark of attention as it plays its part in psychological phenomena.

Upon the envisagement of a problem-situation such as this—the relationship between the perceptual phenomenon and attention in one of its leading formulations—one must at once cast about for a suitable method of attack. Fortunately, in the present instance, a crucial point is immediately presented, since what has with some authority been maintained as the simplest type of perceptual experience has also been employed to demonstrate the fundamental nature of attention in the theory to be considered. This is the experience of figure and ground. In the *Gestalttheorie* a figure standing upon its ground is held to be the most primitive of perceptual phenomena. Koffka, in a discussion of perceptual theory and the foundations of *Gestaltpsychologie*—the same in which he has inveighed against the attentional concept—has expanded this point in regarding the figure-ground experience as a simple, elementary *Gestalt*.[2] Titchener, some time ago, employed what is in essence the phenomenon of reversal of figure-ground fields to illustrate the fundamental aspects of attention. Here direct controversy enters, for Koffka made specific his general objection to attention by critical reference to Titchener's use of it in relation to figure-ground perception. The objection of Koffka carries the greater weight because it follows upon Rubin, who in his classical study of figure and ground voiced the opinion that theories of attention lend no assistance in accounting for this type of perceptual phenomena.[3] A consideration of Rubin's objections and criticisms can wait until after a careful examination of the theory of attention as clearness of conscious content.

This attempt of Titchener to employ attention as a descriptive concept with clearness as its cue is not without its historical background, for Wundt in particular has had a good deal to say about *Klarheit*, and a clear-obscure differentiation of the contents of consciousness has had at least casual mention by a great host of authorities. By the historical prominence of the clearness-theory Titchener considered his position strengthened: he pointed out that clearness is "the first thing that men lay hands on, when they begin to speak about attention."[4] "Clearness," he went on to explain, "varies between a liminal value and the maximum set by the limit of attentional concentration." It is one of the intensive attributes of sensation, the attribute that distinguishes the "focal" sensation from the "marginal." It is an independent attribute, moreover; in a measure it may

[2]Kurt Koffka, Perception: an introduction to the *Gestalt-Theorie*, *Psychol. Bull.*, 19, 1922, 553-570.

[3]Edgar Rubin, *Visuell wahrgenommene Figuren*, 1921, 96-101.

[4]E. B. Titchener, *The Elementary Psychology of Feeling and Attention*, 1908, 171-188, esp. 186.

vary independently of the other attributes.[5] All sensations and images are more or less clear. Feelings, however, lack this attribute of clearness: they cannot be attended to. Any given consciousness contains two levels of clearness, central and obscure: this is the "dual-level" formation of consciousness—and it is this formation that is found in correlation with the figure-ground differentiation, as is shown by the following illustration:

"Take one of the familiar picture puzzles, a picture which represents, we will say, a house and a garden, and somewhere in which is concealed the outline of a human face. As you search for the face, the contents of the whole picture are at the conscious focus. Suddenly you find it: and what happens? Why, as you do so, the picture drops clean away from the focus; the face stands out with all imaginable clearness, and the house and garden are no clearer than the feel of the paper between your fingers. . . . There is no poising of the picture, after the riddle has been read, midway between crest and base of the wave of consciousness."[6]

In his discussion of this theory of attention Titchener was careful to state that he was dealing with clearness of the attributive variety; such clearness must not be confused with *cognitive* clearness. A tachistoscopic field, *e.g.* may be observed under concentrated attention and thus have a very high degree of sensory or attributive clearness, but the observer may be unable to 'make out' anything at all. What he failed to do was to 'cognize.' Cognition is not attentional clearness, but "an associative process of the assimilative kind."[7] In a later statement Titchener made the distinction between the attributive and cognitive types of clearness even more explicit:

"We must, above all things, distinguish between attentional clearness and intrinsic clearness of definition,—sharpness, precision, cognitive clearness. A process may be transversing the very centre of consciousness, and therefore from the point of view of a psychology of attention may be maximally clear: yet it may be so weak, so brief, so instable, that its whole character is vague and indefinite."[8]

Titchener's distinction of sensory and cognitive clearness was given experimental confirmation in a study by Dallenbach, a study expressly devised to substantiate the concept of sensory clearness in the face of a severe attack by Britz.[9] In a tachistoscope Dallenbach exposed visual stimuli consisting of white cards on which were pasted 5 (in the second series, 6) colored rectangles, and the *O*s were instructed not only (a) to name and describe what they had seen, but also (b) "to give as exhaustive a description as possible of the processes involved during the course of the experiment, particular emphasis being placed upon the report of attributive clearness."[10] The first part of the instructions was expected to evoke a *cognitive* report, the second part a report on *process*. The task set by this dual instruction was difficult, and the *O*s found it trying to attempt to distinguish between attributive and cognitive clearness. In spite of this difficulty, and of other considerations which presently will be noted in detail, Dallenbach found his results to confirm his and Titchener's

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[5] *Ibid.*, 219.
[6] *Ibid.*, 228-9.
[7] *Ibid.*, 238.
[8] *Experimental Psychology of the Thought Processes*, 1909, 17. It is worthy of note that this position that experience may be regarded in two different ways—from the point of view of description (sensory clearness) and from the point of view of cognition (cognitive clearness)—is but one phase of a duality that pervades Titchener's system, a duality of process and meaning, description and statement of meaning, and—as here mentioned—sensory and cognitive clearness. See his *Beginner's Psychology*, 1915; also Description *vs.* statement of meaning, this JOURNAL, 23, 1912, 165-182.
[9] K. M. Dallenbach, Attributive *vs.* cognitive clearness, *J. Exper. Psychol.*, 3, 1920, 183-230. Cf. C. A. Britz, *Eine theoretische und experimentelle Untersuchung über den psychologischen Begriff der Klarheit*, Saarlouis, 1913.
[10] Dallenbach, *op. cit.*, 187.

original position that sensory or attentional clearness is a descriptive affair to be distinguished from the clearness of cognition.

Rubin's objection to attention and clearness as pertaining to figure-ground perception was broad enough to include both the types of clearness defined above. He objected to the use of attention in Wundt's sense, and the argument is applicable in the same way to Titchener's attentive clearness (though Wundt's *Klarheit* was not so refined a term). A given field of an equivocal stimulus, after reversal so that it is perceived as figure, has not by that fact changed from a low to a high degree of clearness; it is now a particular experiential object, an object which before the reversal was not in consciousness at all. Or, in other words, the change of a field from ground to figure is not a change of a single object from low to high clearness-status: a field as figure is one thing, and the same field as ground is another. Rubin maintained further, in explicit criticism of Lehmann's use of attention as degree of cognition, that figure-ground reversal is in no better case in being explained as a change of cognition. If, with an equivocal stimulus, a field is at one time perceived as figure, and at another as ground, there is in the typical instance no resemblance whatever between the two experiences; the experiences can not be regarded as one affair with two phases differentiated by the degree in which they are cognized, simply because cognition characteristically indicates no relationship, no connection, between the two. If a stimulus has been, perhaps many times, perceived in one manner, and is later presented so as to be experienced with the fields reversed, it is not ordinarily recognized as having ever been seen before.[11]

This is significant criticism, but the question cannot be regarded as settled; it is possible enough that the bearing of clearness-relations upon perceptual experience is not crucially shown in the phenomenon of reversal of figure-ground fields. Rubin can be right in the contention that reversal is not to be explained primarily on the basis of an exchange of clearness-status, yet at the same time the clearness-differentiation of conscious contents in a simple perceptual experience (as, *e.g.* figure and ground) may be a very real and significant affair. The figural field may always, or nearly always, stand on a high plane of clearness as compared with the groundal field, yet in a reversal the situation may be so complex that the change of clearness-relations looms low in the total experience and therefore readily escapes any but the most careful scrutiny.

This whole situation points to the need of experimental investigation. There is needed, before any solution can be admitted, a more careful understanding of the nature of clearness. Furthermore, the problem should be reduced to its simplest terms. Instead of considering clearness-relations as they obtain in the perhaps rather complex matter of reversal of figure-ground fields, one should first investigate the comparative clearness of figural and of groundal fields as these are experienced in close contiguity. The primary question, and the question upon which the following experimental series are directed, is whether there is an experiential difference in the clearness of the two fields in figure-ground perception. A problem that bears immediately upon this one is that of the precise nature of clearness; one must know, for example, if there really are two kinds of clearness, which of them is pertinent to the figure-ground phenomenon, and how in this relation it is to be distinguished from the other. Incidentally, an investigation of clearness in its relation to figure-ground experience ought to be of some interest in regard to the dual-level theory of clearness: if in the first place a simple figure standing forth upon its ground can be shown to be the fundamental type of perceptual experience, and in the second place figure-ground status can be correlated with high and low degree of clearness, then it is apparent that the theory of the two levels is placed in a peculiarly significant position as the formulation of a fundamental differ-

[11]Rubin, *loc. cit.*

entiation of consciousness. The primary problems, then, that this experimental investigation will hold up for solution, are three in number: (1) the pertinence of attention as the clearness of experiential content to a simple perceptual phenomenon; (2) the nature of this clearness, with particular reference to the sensory-cognitive distinction; and (3) the validity of the so-called "law of the two levels."

I. Clearness of Figural and Groundal Fields

Series 1: Relative judgments. It was desired in this series to present successively two stimuli which could beforehand be determined to be perceived each in a particular manner—with a given field as figure—so that the subject could be required to compare as to clearness the figural field in the one stimulus with the ground in the other, or the figural field in the one with the figural field in the other, or similarly the ground in the one with the ground in another. This was made possible by the fact, which had appeared in preliminary investigation, that all the *O*s, under passive instructions and under the instruction to attend to black, when presented with stimuli with one field black and the other white, got an overwhelming predominance of black figures. Therefore, with the quality of the figure determined, it was only necessary, in order to obtain the desired comparisons of figural and groundal fields, to control the judgment by position, and then to vary the combination of stimuli at will. This procedure will be clear upon an understanding of the form of the stimuli.

The stimuli were developed with view to equivocality. One of the factors in equivocality is equality of the areas of the two stimulus-fields. The stimuli consisted of white cards upon which were pasted black strips so that, when the cards were placed in the exposure-apparatus, the visible area was divided vertically into right and left fields, one black, the other white; in a central area, 1.5 x 1.5 cm., the line of separation underwent various convolutions. The visible stimulus-area was 3.7 cm. wide and 6 cm. high, and was framed by an intermediate gray 1.5 cm. in width. This gave a stimulus that could be perceived either as black protrusions on a white ground or as white extensions on a black ground.[12] Black occurred as often on the left as on the right. The first arrangement (black on left, white on right) is symbolized as 'Bk-W,' the second as 'W-Bk.' The preparation of the stimuli was standardized by a stencil which marked the general boundaries of the cutting, so that after the black strip was pasted on the white card the convolutions were centrally situated, and, typically, extended in about equal areas to the left and to the right of the central vertical axis. The convolutions of no two stimuli were the same. A stimulus was presented to a given *O* but once.

The stimuli were exposed in pairs in a form of the Dodge tachistoscope;[13] the instrument permitted successive presentation of two objects, and also control of the fixation-field. The time of exposure was regulated by means of a pendulum operating through contacts situated along the arc of its excursion. As sources of light, two 150-watt high-speed lamps were employed for the exposure-fields and a 75-watt Mazda lamp for the fixation-

[12] Cf. Rubin's Figs. 2 and 13, in his *Visuell wahrgenommene Figuren.*

[13] See E. G. Wever, Figure and ground in the visual perception of form, this Journal 38, 1927, 200 f. This instrument was a modification of that described and figured by H. R. De Silva, this Journal, 37, 1926, 470-471.

field. Intensities were equated by means of diaphragms. The fixation-field consisted of a light-gray field with a black vertical and a white horizontal cross-line.

At a signal *O* placed his head in the hood of the instrument and fixated the cross lines. The fixation-field was then removed, and 0.3 sec. later the first stimulus was exposed for 0.2 sec. After another 0.3 sec. interval the second stimulus was exposed for 0.2 sec. After 0.23 sec. the fixation-field was replaced.

The *O*s in the experiment were Mr. K. E. Zener (Z), Assistant in Psychology, Mr. J. G. Beebe-Center (*B*), Tutor in Psychology, and Dr. F. A. Pattie (*P*), National Research Fellow in Psychology. All were experienced observers.[14]

The subjects were instructed as follows:

"At the signal 'Ready,' fixate the point of intersection of the cross-lines. After the word 'Now,' will be presented a pair of stimuli. For each of these two stimuli your attention is to be concentrated upon the *black* stimulus-field.

"You are to make a relative judgment of the clearness of the *left-hand* fields. A report of 'much greater' (or 'greater') means that the clearness of the second left-hand field greatly exceeds the clearness of the first left-hand field. A report of 'slightly greater' means that the clearness of the second left-hand field is slightly greater than the clearness of the first left-hand field. A report of 'equal,' 'slightly less,' and 'much less' (or 'less') means that the clearness of the second left-hand field is equal, slightly less, or much less than the clearness of the first left-hand field.

"Clearness, often called sensory clearness, attributive clearness, attensity, and (sometimes) vividness, is to be taken as a designation of that property which distinguishes the focal mental process from the marginal. It is not to mean sharpness of outline, or distinctness of demarcation, or value of intensity-interval, unless (and only insofar as) these properties are inseparable."

The results are shown in Table I. There are two sets, A and B, of 100 judgments each, equally divided among the four arrangements of the comparison pairs: (1) Bk-W, W-Bk, (2) Bk-W, Bk-W, (3) W-Bk, W-Bk, and (4) W-Bk, Bk-W. Assuming that Bk was always figure, this gave clearness-comparisons of figure with ground, figure with figure, ground with ground, and ground with figure.

That black was figure was given careful checking. The *O*s were instructed that they were expected to get Bk as figure in all cases and were to report immediately whenever W appeared as figure in either or both of the presentations, at which the judgment was thrown out. In set A, *Z* reported 7 such cases, *B* and *P* none; in set B, *Z* reported 17, *B* 2, *P* none. Further, the clearness-series were always preceded by a short series in which the *O* was asked to report merely which field was figure and which was ground in the two cases. Counting these preliminary series, *Z* reported 25 W figures out of 311 pairs, *B* 5 out of 302, and *P* none out of 364. In spite of these precautions it is possible, though improbable, that one or two undetected cases occurred in which W was figure either wholly or in part.

The table shows for every *O* the actual number of judgments under each of the five categories for each set, as well as the

[14]They had acquired special training in regard to the figure-ground phenomenon in a previous experiment. See Wever, *op. cit.*, 194-226.

TABLE I

Comparison fields	O	Set	Clearness of second field				
			'much greater' or 'greater'	'slightly greater'	'equal'	'slightly less'	'much less' or 'less'
(1) Bk-W	B	A				1	24
(2) W-Bk		B					25
		%				.02	.98
First field is Figure;	Z	A		1	1	10	13
		B				4	21
		%		.02	.02	.28	.68
Second field is Ground	P	A					25
		B			7	14	4
		%			.14	.28	.58
(1) Bk-W	B	A		2	12	11	
(2) Bk-W		B		6	17	2	
		%		.16	.58	.26	
Both fields are Figure	Z	A	1	4	14	7	
		B	.02	7	14	3	
		%	.02	.22	.56	.20	
	P	A			25		
		B			25		
		%			1.00		
(1) W-Bk	B	A		4	21		
(2) W-Bk		B		10	14	1	
		%		.28	.70	.02	
Both fields are Ground	Z	A		9	8	7	1
		B		15	9	1	
		%		.48	.34	.16	.02
	P	A			25		
		B			25		
		%			1.00		
(1) W-Bk	B	A	23	2			
(2) Bk-W		B	25				
		%	.96	.04			
First field is Ground;	Z	A	10	13	1	1	
		B	25				
		%	.70	.26	.02	.02	
Second field is Figure.	P	A	25				
		B	22	3			
		%	.94	.06			

percentages for both sets taken together. The 'slightly greater' and the 'slightly less' categories are very close to the 'equal;' these were inserted because it early became evident that some distinction must be made between the enormous differences of clearness experienced when one field was figure and the other was ground, and the minor differences when both were either figure or ground.[15]

[15]There is evidence that *P* did not make this finer distinction. His 'slightly less,' *e.g.* is near the 'less' category.

The results show that for all *O*s a figural field is, in the great majority of cases, judged as more clear than a groundal field. In no instance was a figural field judged less clear than a groundal field. In only two instances was a figural field judged as slightly less clear than a groundal field, and in nine instances equally clear. Minor differences of clearness are shown when both fields were either figure or ground.

Frequent protocols were taken, and the *O*s were required at every session to characterize clearness and the judgment of clearness. Valuable data were obtained on the nature of 'sensory' clearness; they will be considered after the next series in the general discussion of clearness.

Series 2: Absolute judgments. The data of the first series showed plainly that the clearness of figural and of groundal fields is a reportable affair, and that the relative clearness of these fields follows a general tendency, namely, that of the higher clearness-status of figural fields; the protocols revealed further the pertinence of clearness-relations to an account of the figure-ground phenomenon and something of the precise nature of clearness itself.

This second series was arranged for a number of reasons. In the first place, it was desirable to inquire further into the matter of clearness; and in the second place, it is always of interest to compare with obtained data the results of a new procedure involving somewhat different stimuli and an altered manner of presentation and report. Moreover, it must be plain that the results of the first series show the clearness-relations of figural and groundal fields in general, and not in any specific case, because the fields compared were always of different, successively occurring experiences, and not of a single experience. From such data one can with safety argue that in general the clearness of a figure will be greater than that of its ground, but one can not decide whether every figure is clearer than its ground or whether the relation is a general but not a necessary one. For the determination of this point the method must be more direct.

The stimuli were of the same form as before, but were wholly altered in quality: here one field was blue, the other orange-red (symbols 'b' and 'o'). The stimulus cards were prepared by pasting a 'b' strip on an 'o' square, or an 'o' strip on a 'b' square, and affixing the square to a card of a form to fit the object-holder of the tachistoscope. Blue appeared equally often on the left and on the right. Milton-Bradley 'spectral' standard blue and orange-red colored papers were used. The chromas appeared the same; the blue was the darker.

The stimuli were presented singly in the instrument used for Series 1, but with one channel unused. The fixation-field and other conditions of exposure were controlled as before. Reports were taken in continuous fashion, with a pause of about 1 min. after every ten reports, thereby out-

lining a 'group;' a rest-period of about 5 min. followed each set of four such groups. The stimuli presented and the 'attitudes' under which they were observed were haphazardly determined.

The instructions were as follows:

"Under a given attitude (announced by the experimenter) observe the presented stimulus, and report (*1*) whether the experience was 'o' figure, 'b' figure, 'o' mixture, 'b' mixture, 'o-b' alternation, 'b-o' alternation, 'o' with 'b' alternation in image, 'b' with 'o' alternation in image, both, or neither; and (*2*) make an absolute judgment of the clearness of each of the two fields according to the following scale: '9' maximally clear; '8' very clear; '7' clear; '6' fairly clear; '5' fair; '4' fairly obscure; '3' vague; '2' very vague; and '1' absolutely obscure."[18]

The first part of the instructions will bear some explanation. Preliminary investigation had indicated, and the first series had shown plainly, that figure-ground experiences are of many types, ranging all the way from the perception of a simple figure standing upon its ground to the perception of a mere flat pattern of qualities involving no figure-ground differentiation at all. Between these extremes stand the intermediates, or 'mixtures,' in which both objective fields possess the property of figure but one strongly or slightly predominates. A 'blue mixture,' for instance, is such an experience in which portions of both the 'b' and the 'o' fields enter into the figural experience, but the 'b' predominates; the ground is made up typically of the peripheral portions of both fields. At times a reversal occurs during the period of presentation, so that, *e.g.* an experience of 'b' figure on 'o' ground may give way to an experience of 'o' figure on 'b' ground ('b-o' alternation). Such alternation may take place in an image immediately following the presentation (when it is named a "'b-o' alternation in image"). At times both 'b' and 'o' fields possess equally the property of figure (reported as 'both'); and, as just suggested, figure-ground differentiation may fail, giving a flat mosaic or pattern (reported as 'neither' or 'none').

The attitudes under which the stimuli were observed had been defined as follows:

"Unless before the presentation of a particular stimulus you are expressly instructed to the contrary, you are to (*1*) keep fixation central; (*2*) keep attention central; and (*3*) keep attitude (set, expectation) passive. You will be reminded frequently of this set of conditions by the words 'Passive Conditions.'

"Frequently, before a given presentation, the experimenter will modify the above instructions in one (or more) of the following ways:

"(1) (a) 'Fixation Left.' (Fixate the mid-point of the left horizontal line of the fixation-field, and maintain that position during the presentation.)

"(b) 'Fixation Right.' (Fixate the mid-point of the right horizontal line, and maintain that position.)

"(2) (a) 'Attention Left.' (Attend to the left-hand field.)

"(b) 'Attention Right.' (Attend to the right-hand field.)

"(3) (a) 'Left-Figure Attitude.' (Try to see as figure the left-hand field.)

"(b) 'Right-Figure Attitude.' (Try to see as figure the right-hand field.)

"(c) 'Black-Figure (or White-Figure, or any other quality of figure) Attitude.' (Try to see as figure the field of the quality named.)"

[18] Adapted from L. R. Geissler, Measurement of attention, this JOURNAL, 20, 1909, 511.

The main function served by the various attitudes of observation was the prevention of the operation of a single *Einstellung*. If the *O* is left to his own devices, or if a single attitude is instructed for, he very soon is gripped by a most powerful determination through which all or nearly all of the presented stimuli are perceived with the same quality of field as figure, and everything that is gained in making the stimuli equivocal is thereby lost again.

The *O*s showed themselves able to make the desired judgment. They maintained, however, that while as *relative* indices of the clearness of fields within a given consciousness the judgments were reliable, the *absolute* values of the judgments of clearness were not of great certainty. Therefore the data have been regarded merely in the relative sense. The results from the presentation to each *O* of 40 groups of stimuli are shown in Table II. Alternations and the occasional failures do not appear in the table.

TABLE II

Field clearer	*O*	Nature of the experience and direction of attitude														
		'b' figure			'b' mixture			both or none			'o' mixture			'o' figure		
		b	*no*	*o*	*b*	*no*	*o*	*b*	*no*	*o*	*b*	*no*	*o*	*b*	*no*	*o*
'b'	*B*	75	50	80	25	12	10	1	1	1	0	0	1	0	0	0
	P	78	78	24	7	6	2	1	2	2	0	0	0	0	0	0
	Z	18	2	1	0	0	0	1	0	0	0	0	0	0	0	0
'b'and'o' equal	*B*	2	3	7	6	7	8	1	5	3	1	1	0	0	1	0
	P	14	22	51	3	7	6	11	20	11	2	0	0	0	1	0
	Z	3	0	0	1	0	0	3	0	2	0	0	0	0	0	2
'o'	*B*	0	0	1	1	11	5	2	4	1	8	4	5	13	0	9
	P	0	0	9	0	0	4	0	6	10	0	2	4	1	0	0
	Z	1	0	0	0	0	3	3	0	2	5	1	1	85	95	131

The first row of the table shows the nature of the experience as reported. The second row indicates the field toward which the attitude had been directed: *b* indicates that the attitude was directed toward the blue, *o* that it was toward the orange-red, and *no* that attitude was passive; aside from this no differentiation is here made between the various attitudes of observation. The remainder of the table shows, for each *O*, the actual number of cases, for every kind of experience, that the 'b' field was more clear, that the fields were equally clear, and that the 'o' field was more clear. The 'none' cases were infrequent, and since they showed as to clearness no differences from 'both' cases, they were thrown in with the latter.

The table shows plainly a prevailing tendency for the figural field to be judged as more clear. As the first and last divisions of the table show, the groundal field was more clear in but 11 cases out of 927, of which 10 occurred when the attitude had been directed to the field which was experienced as ground.

Cases of equal clearness were more frequent, especially where the field experienced as ground was that toward which the attitude had been directed. In the experiences of 'b mixture' and 'o mixture' the prevailing tendency was for the clearness to be greater for that field reported as dominant in the figural experience. The distribution of clearness in the 'both' and 'none' cases was fairly even, with a predominance of cases in which both fields were of the same clearness.

TABLE III

O	Probability that		
	figural field is more clear	the fields are equally clear	groundal field is more clear
B	.9419	.0539	.0041
P	.6511	.3166	.0324
Z	.9822	.0148	.0030

The next table (Table III) is computed from the data in the first and last divisions of Table II. It gives the probability,[17] for each *O*, that the figural field is judged more clear, equally clear, or less clear than the groundal field. The probability is high that a figural field occupies a clearness-level higher than that of its ground. There is a small probability that figural and groundal fields occupy the same level of clearness. The probability that a groundal field is clearer than the figure with which it occurs is extremely slight.

The general tendency shown by these results is obvious enough, but the situation must not be made to seem too simple; certain facts of the experimental setting must be considered. The various figural experiences do not come already catalogued to the *O*, do not fall into discrete groups, but vary by innumerable gradations from a most definite experience of unified clear figure standing forth from undifferentiated ground to the experience of a plane pattern. Between stand the 'mixtures,' in which portions of both fields have figural properties, 'mixtures' ranging from a type in which one quality definitely dominates to one in which both qualities are of like figural value. Frequently two 'figures' are inextricably interwoven, several levels of localization are present, and the various parts show differences in the area of the experience which they involve. Difficult, therefore, is the task of the *O* in reporting his experiences in any arbitrary, necessarily limited, set of categories. When called upon to report the clearness of 'the two fields,' in all except the pure figure-ground experiences he is in a difficult position. In the cases of intermediates there are not two fields frequently, but

[17] The 'empirical' probability, computed by the formula, $a/(a+b)$, where a = the number of times the event occurred, and b = the number of times it failed.

three or four or five different fields. The *O* in this latter situation may do one of two things: he may give a gross judgment of the 'average' clearness of the 'o' and 'b' areas, or he may designate in detail the clearness of the various parts. In the above experiment the *O*s were allowed to follow their own inclinations in this matter: *P* gave 'average' judgments, *B* and, to a somewhat less extent, *Z*, reported upon the clearness of the separate parts, usually designating the principal fields.

This produced a minor difficulty in the preparation of Table II, for it was necessary to form for the record in the table a rough numerical average of the clearness of the separate areas reported. The difficulty is present because it is questionable whether this does justice to the data; but it is a minor difficulty because cases of this kind were relatively few, and the alteration of the data by their complete omission would be hardly detectable.[18]

A more serious difficulty in the experiment arises from two considerations. In the first place, it seems that in general, but in the intermediates particularly, the portion of the groundal field adjacent to the figure is perceived as more clear than the peripheral portion. In the second place, there was shown a constant tendency for the more clear portion of the groundal field to be the cue for the judgment of groundal clearness, while the less clear portion failed to enter, either separately or in the gross, into the determination of the judgment. These things, taken concurrently, mean that there exists a general tendency, over a number of cases, for the groundal field to be judged as having a higher clearness-status than it would have if its periphery were involved. The situation is simply that the periphery in many cases is not such as to elicit report. The effect of this tendency upon the data, if it has any effect at all, is to reduce the frequency in which the clearness of figures is found to be greater than that of their grounds. Since this frequency has been shown to be very great under even the highly equivocal conditions of this experiment, it follows that such a tendency certainly has not been operating with any great effectiveness; yet these data are not such as to remove the opportunity of arguing that the failure to evince an invariable relation between clearness-values and figure-ground status is due to the operation of some such factor. If this were the case, however, one would expect to discover evidence of the operation of the factor in the protocols; nevertheless an examination of the protocols revealed no such evidence—in fact, it showed evidence which substantiates the data at their face value. There were convincing cases,

[18]Of really equivocal cases there were but 8, so that the results can have been subjected to but little violence by this arbitrary averaging procedure. It will be recalled that *P* did his own 'averaging;' with this *O* the difficulty is the same, though not so well known.

few in number to be sure, but, it would seem, none the less crucial, in which the groundal field was reported as clearer than the figure, yet the report showed the groundal field as a whole to function for the report. Such a case is the following, in which an experience was reported as a 'b' figure on an 'o' ground, yet with 'o' on a higher level of clearness:

"Blue figure; 'o' clearer. The 'o' seemed to take up the main part of the field. My attention was on the 'o.' The 'b' outline was very indefinite. The figure was sort of jagged, and it lay above the 'o.' " (*P*).

It would seem, then, from the evidence, that the relation that has been shown between clearness-degree and figure-ground status in a perception is merely what it has been called in this report, namely, a general tendency, and not an invariable relation. This merely adds weight to the contention that in any complete account of figure-ground phenomena the clearness-relations must be set forth; if the variation of clearness were univocally dependent upon figural or groundal status it might be neglected in a description without serious consequences, since for any field or part-field it would be necessarily involved in the assignment of figure-ground status. However, as here is shown, the probability is that such is not the case, and one therefore to be on the safe side of adequacy must include in a description of figure and ground a report on clearness-relations.

Extracts from the protocols of both *Series 1* and *2* will serve to illustrate this point of the pertinence of attentional status to an account of figure-ground phenomena and to indicate as well something of the nature of the various possible figure-ground experiences.

(*a*) *The double-field figure.*

" 'Both.' An experience of two 'b' blobs, fairly far apart, on an 'o' background. They stood out. Then one 'o' blob on a 'b' background; that stood out" (*B*).

" 'Both.' There were two groups, one of Bk into W, and one right below it, W into Bk, and the two prongs both seemed to overlay the other fields. The big blotches of Bk and W seemingly were on the same plane. No predominating quality" (*P*).

" 'Both.' The thing seemed to be two members of one pattern. The contour seemed to belong as much to one as to another. It looked like a matter of the fingers interlocking, not interlying. The 'o' was clearest" (*P*).

" 'Both.' The clearness was exactly the same. The attention was greatest along the contour. There was a little prong of 'o' into the 'b.' That was the clearest thing in the field" (*P*).

" 'Both.' W fingerlings and Bk; both distinctly figure" (*Z*).

(*b*) *The experience of pattern.*

" 'None.' The experience was of a mosaic of 'b' and 'o.' The central part of the experience was more clear and more outlined. But aside from that there was no distinction of fields. A plane, in which the different portions were distinguished by color but not by spatial position with reference to that plane" (*B*).

" 'None.' Simply a bunch of 'o' and 'b' striations across the field, the 'o' being thicker. The clearness of the whole central area was rather high. The 'o' was slightly brighter. There wasn't any definite contour. Neither was definitely a unit" (*Z*).

" 'None.' The field consisted of 'o' and 'b' strips; rather unstable; no definite contour bounding the thing. Neither one came out as figure. The 'o' was brighter than the 'b.' It seemed to possess slight figure-quality, but no definite contour developed. Definitely on the same plane of localization. The main thing is lack of definite contour" (*Z*).

(*c*) *The intermediate experience.*

" 'b mixture.' The 'b' experience was central. There was a 'b' bump which was central and stood over everything else except a little 'o' blob that stood out terrifically over everything. The rest of the 'o' vanished more or less" (*B*).

" 'b' mixture.' An experience of two 'b' tongues standing out against an 'o' background, one in the upper, one in the lower left quadrant. The 'o' blob stood out against the 'b' background in the middle of the left half of the field. The 'standing-outedness' of the 'b' was greater where it stood out than the 'standing-outedness' of the 'o' where it stood out" (*B*).

" 'b mixture.' Both fields seemed to have figure-quality, but the 'b' overlapped the 'o,' especially with respect to one long prong which seemed to stand out very much—'b' seemed to have the upper hand" (*P*).

" 'o mixture.' General experience of 'o' on the right, of 'b' on the left. The most figured thing was a little 'o' blob slightly below the horizontal cross-line. It stood out from everything else; was very sharply contoured. Above that was a little 'b' blob which stood out from the rest of the 'o' and the rest of the 'b.' It was figure too" (*B*).

" 'o mixture.' There the 'o' was clearer and appeared localized a little bit ahead of the 'b,' yet the 'b' seemed still to be figure. On the extreme left some of the 'b' was not seen; seemed shady, fragmentary. The contour was good" (*P*).

" 'Bk mixture.' Central, in the middle of the field and in attention, was an experience of a Bk blob distinctly superposed on a W background. To the left were two smaller blobs of W, superposed over Bk" (*B*).

" 'Bk mixture.' The W took some time to come up to the clearness that it finally had. I saw the W localized a little bit in front of the Bk. The Bk didn't seem to extend behind the W. I had a sort of simultaneous apprehension of the contour of the W and the contour of the Bk" (*P*).

" 'W mixture.' Central was a W tongue on a blackish background. Below that was a Bk tongue sticking over in front of the W background. The W was central and more impressive" (*B*).

(*d*) *The experience of simple figure.*

" 'b figure.' There was a 'b' blob that was the central thing of the whole experience. That stood out on a background of 'o,' and graduated out on a background of 'b' " (*B*).

" 'b figure.' The 'b' field wasn't half the field; wasn't localized very well in front of the 'o.' The 'o,' probably because of larger expanse, was on the same level of clearness as 'b.' The figure was rather small, pressed over toward the right half. It extended from the side to two-thirds the distance to the half-way line" (*P*).

" 'o figure.' There were three levels: the highest level of clearness was a little 'o' blob on a 'b' background. The 'b' blob itself was on an 'o' background for the rest of the experience" (*B*).

" 'o figure.' The 'o' figure uniform, continuously contoured. Differences in localization between figure and ground. Considerable difference in intensity: figure brighter. The figure was surfaced, though rather voluminous rather than glassy. The ground was more filmy; unclear" (*Z*).

" 'o figure.' The 'o' part figure; 'b' ground. The texture of the two was not very different. Very slight localization of 'o' in front of 'b.' The 'o' seemed to be contoured, while the 'b' wasn't; 'o' was more definitely a compact unit—that was the main thing. Brightness in each field was even; 'o' was more *eindringlich*. It was not a particularly good figure" (*Z*).

" 'Bk figure.' A rather filmy whitish experience on the left; on the right a Bk, definitely localized experience with little blobs sticking out into the W. The W was both filmy and translucent. There was a certain depth, not thickness" (*B*).

" 'Bk figure.' Bk figure on the right; perfectly definite contour. On the left was nothing but 'fuzz'—*i.e.* ground—not localized." (*B*).

" 'W figure.' Figure was W; was on the right; and clearest in attention. Bk was relatively unclear" (*P*).

" 'W figure.' W figure with jagged edges. A deep Bk inlet on the main part of the figure seemed almost part of the figure. There was a jagged W in front of the Bk, and then this Bk inlet over the W" (*Z*).

(*e*) *The experience of alternation.*

" 'b-o alternation.' The 'b' seemed just for a moment to be figure, but the 'o' swelled up and the 'b' retired in space. The end of the experience was a distinct 'o' figure, with 'b' in the background" (*P*).

" 'Bk-W alternation.' Experience of a Bk object, or surface, with some recessions, and in that recession some pseudopodia over a W background. Background not focal in any sense. Then some of the W between the pseudopodia seemed to overlap the pseudopodia. The rest of the Bk area remained figure, except the pseudopodia that seemed to recede while the W came forward" (*B*).

" 'Bk-W alternation.' The Bk seemed to be figure for just a moment, then the W, especially where it jutted into the Bk, seemed very prominent, and the Bk seemed to drop back to the background" (*P*).

" 'Bk-W alternations.' The Bk figure consisted of the protruding Bk portion; it was quite circumscribed. It had nothing to do with the rest of the field, was of surface-character; the ground was film. Indefinite localization, but quite uniform texture. Then the part of the W field immediately below this Bk portion assumed surface-texture and became a good deal brighter. In both parts both Bk and W stood out. Part of the W field became figure. I don't know what happened to the contour along that portion that belonged to the Bk and W portion" (*Z*).

" 'W-Bk alternation.' The W figure was very brief and not complete; a certain W protrusion was brighter than any other part of the W—it was the clearest part of the field. Then a Bk finger differentiated itself and came out, had surface-texture, was continuously contoured; and the W seemed to recede and grow more film-like" (*Z*).

Alternations were reported by all *Os*; in most cases the *O* was able to report the clearness of the fields in both stages of the experience. The most frequent failures in such reports were for the groundal fields. The general tendency was for the clearness of the second figural field to be greater than that of the first figural field. This relation held also for the groundal fields, in the same order. Without exception, the figural field for each stage was reported as clearer than its ground, or the clearness of the ground could not be reported at all.

An intimate relationship has been demonstrated between figure-ground differentiation and the clearness-degrees of the visual areas. The results of the two series are in agreement. A field which is perceived as figure stands in general on a clearness-level higher than that of a simultaneously or successively perceived ground. In an intermediate experience portions of

both fields are likely to be figure, of a high, and approximately equal, degree of clearness, while the ground consists of the remaining areas of the two fields and is on a lower level of clearness. Reversal of ambiguous stimulus-fields is found to be accompanied by a reversal of the order of clearness-levels. Figure and ground are thus respectively correlated with higher and lower degrees of clearness.

II. Sensory and Cognitive Clearness

It is time, after so much has been said about clearness and its pertinence to the figure-ground problem, for something specific to be recorded as to the nature of clearness itself. Since it is of apparent necessity to know what is the clearness that one is dealing with, let the first approach be an examination of the sensory-cognitive distinction for clearness.

In this experiment there was found no intelligible distinction of sensory and cognitive clearness. As the instructions show, the *O*s were expressly warned against a confusion of these supposedly existing affairs, and all the presumption was in favor of a distinction, but nevertheless a distinction failed to appear. The conclusion from the results is that in this experiment, and so far as the figure-ground phenomenon is concerned, the differentiation of attributive and cognitive clearness does not exist. The clearness obtained in this experiment is no more what has been called "sensory" than it is what has been termed "cognitive;" it is simply that property which distinguishes focal and marginal mental processes, a property which can be reported by an observer as a portion of a figure-ground experience, and a property which correlates highly, but, according to the evidence, not absolutely, with figure-ground status in a perception.

The nature of this clearness is shown in detail in the reports of the *O*s. The *O*s had very frequently been asked, during the preceding series, to deliver introspective reports as to the nature of clearness and its pertinence to the experiences judged. Their remarks are recorded in detail:

B: "For me at present, clearness is definite value with respect to reportable attributes. The greater the number of such attributes that have a value in the experience, the greater the clearness. I might say that it is 'figurehood,' but I am wary of that as 'blanketing.' It is a definite experience that I can place in the scale of hue, intensity, etc. If it isn't clear I can only roughly localize it; its hue is vague; its brilliance is variable; homogeneity may also come in. It is *definite* things; you can only approximately describe the ground." "The field that is more clear is more brilliant, has some characteristics of texture, etc. An unclear field is 'fuzzy.' An unclear field is on the fringe of the picture. The clear field I can say something about." "That object is more clear which is the spatial reference-point in the experience."

Z: "The more clear field has more structure in it." "Clearness is the presence of something that can be reported upon." "The Bk was not

clear; it was only vaguely present. I don't know that you can say very much about it. I can't say very much about its intensity, because it wasn't in consciousness in a form in which a thing like intensity would be very well represented: it couldn't be reported upon. The farthest, *i.e.* peripheral, ground doesn't seem to be in consciousness at all. The ground near the figure seems to be more clear. The clearness of the figure is uniform to a large extent in from the contour. The ground *near* the contour has a *poor* clearness; that portion away is almost lacking in clearness. The determination for the W seems almost to eliminate the Bk. The only thing that stands out, and can be reported upon, is the W. The Bk is non-existent. Lack of clearness is inability to report." "This clearness-judgment seems a sort of secondary judgment. It is certainly not the same thing as a judgment of intensity; it is a little bit farther removed from the actual properties of the experience as such. It is somehow connected up with the ability to report, with being able to hold the thing and to give a report upon it after it is past."

P: "I could draw the Bk [Bk is figure], but not the indentations of the W, so I call the figure clearest." "The figure is always clearest. . . . . The only thing I could draw would be the Bk part. To draw the W I'd have to sit down and think what was the Bk. That's what I mean by clearness." "Clearness is something like vividness. My attention is caught by the Bk figure always. The thing looks more vivid, more prominent. W doesn't amount to anything." "The Bk was on the right side; Bk was figure, was more clear. My eyes and attention were attracted by Bk. W had no particular quality at all. I see what was meant by the thing-quality of figure. It is as good an experience of clearness as you get anywhere." "Bk had projections somewhat in front of the ground. Bk was figure. I had no knowledge whatever of the projections of the W. I couldn't draw the W; I would have to draw the Bk. Bk was figure and Bk was clearer."

At the end of *Series 2,* the *O*s were requested to give an introspective characterization of clearness. Their accounts are appended:

B: "The characteristics of clearness are *centrality in the field of experience* and *memory-value.* That is to say, that portion of the field which is clear has existentiality in memory-image. Those are the only characteristics of clearness, but there are several characteristics of clear objects: intensity; contoured-ness; localization is space, especially depth; and heterogeneity relative to the rest of the field—large qualitative difference, which may correlate with contoured-ness."

Z: "A thing is clear in proportion as it can be reported upon. If it is indistinct, very little can be said about it." "The thing stands out in the experience—it has figure-quality, it is the important thing in the experience. . . . In the experiential field one part of the field will be distinct, and will seem to stand out more, is more *eindringlich,* like the focus as opposed to the margin."

P: "In that last one the judgment ['o' more clear] meant that I simply saw the 'o' and the 'o' took up most of consciousness; 'b' was less vivid." "The thing just seemed to be the most important."

The focally clear portion of a visual experience is, in effect, that part of the field which stands forth in the experience, which obtrudes itself upon consciousness, which is distinct and prominent above the undifferentiated background, which can be pointed to, grasped, described, reported definitely upon, which persists in memory. The marginally clear portion, on the other

hand, is recessive, subordinate, it slips from one's grasp, eludes description, can be reported but hazily, is soon lost to memory. If in the above description for 'focally clear portion' and 'marginally clear portion' one writes 'figure' and 'ground,' the general features of figure-ground status will be set forth. Here is no mention of cognitive clearness. This is but one kind of clearness.

This result is, of course, in contradistinction to the conclusions which Dallenbach has drawn from his experimental findings. In Dallenbach's experiment, mentioned above,[19] the criterion of 'sensory' clearness was the *O*s own designation of the clearness-status of the various portions of the experience during the presentation. Such reports fell into four classes: (1) no report of clearness; (2) report of one clearness-degree—one level; (3) report of two clearness-degrees—dual-level; and, infrequently, (4) report of three clearness-degrees—multi-level. But the *O*s were also required to describe as fully as possible the experience itself. These 'meaning' reports (so-called) varied, as one would expect, from most complete designations of the nature of the part-experiences—hue, intensity, position, and even texture of the color square—to nothing at all. Fullness of report was then taken by Dallenbach as the criterion of cognitive clearness, and cognitive clearness thereby classified in 17 steps or degrees. For example, report of the form, hue, and tint of a color-square gave degree number 14; if, however, the *O* reported chroma instead of tint the cognitive clearness was of degree number 15. Since, by this procedure, (1) sensory clearness had been given but three or four classifactory steps, with none or one or two real degrees in a given consciousness, and cognitive clearness, on the other hand, had been discovered in 17 different degrees, with many and various steps in a given consciousness, and, moreover, since (2) cases occurred in which a high degree of attributive clearness was reported for a process described very poorly, and in which a low degree of attributive clearness was reported for processes described in detail, Dallenbach came out with the conclusion that sensory clearness is distinct from cognitive.[20]

It is significant that in this experiment in which Dallenbach has justified the distinction between sensory and cognitive clearness he was fearful lest his own *O*s were confusing the two. His fearfulness had a real foundation. He, who was one of the *O*s, said that "neither the method nor the instruction was conducive to good psychological description." Another of the *O*s maintained that "the report is so complicated that it seems foolish to talk about attributive clearness." He had then no criteria for distinguishing attributive clearness from cognitive.

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[20]Attributive vs. cognitive clearness, *J. Exper. Psychol.*, 3, 1920, 204-5.

This *O* was much disturbed by after-images, and believed that relative clearness in the after-images influenced his report of the processes which had occurred during the exposure. The third *O* said that any side of the report could be predetermined without affecting the process of the exposure-period. However, these considerations apparently did nothing to reduce Dallenbach's conviction of the separateness of sensory and cognitive clearness, for in data obtained under these conditions he found proof of the distinction. His *O*s were in error when they reported one clearness for the other. Britz had got one kind of clearness when he should have obtained another. Even recently the faith in the reality of the 'attributive-cognitive error' has shown itself in the literature.[21]

Before entering upon a criticism of Dallenbach's position on this matter one must look to the premises of the whole discussion. These are two in number: the first is that the *O* is able to report upon the clearness of a given part-content accordingly as it stands out to his grasp in the total experiential content; the second is that the clearness of conscious processes determines the fullness of report upon these processes. Dallenbach has stood upon these two premises, but he has read one kind of clearness in the first, and another kind in the second; this means that one goes but part of the way with Dallenbach if he agrees to these premises but refuses to recognize the necessity of any distinction of clearness. That these premises are valid is shown not only by data of Dallenbach's, but also by results from the present experiment. The *O*s were able to render reports upon the clearness of their experiences; and they agreed in maintaining that the essential thing about this clearness is its positive relation to the ability to describe the experiences themselves.

What Dallenbach found in his study, as shown above, was that reports of the 'sensory' clearness of the experiences for the period of presentation did not correlate perfectly with a more or less arbitrary tabulation of the quantity of things that the *O* was able to say in description of the experiences after they were past. But why should the fact that these two things do not give a one-to-one correlation be any proof that the observer has had within his consciousness two types of clearness? The situation may be simply—and this is the present contention—that Dallenbach was dealing with one kind of clearness, but dealing with it by two methods; and that the divergence in the results of these two methods, the divergence from which he has argued for two clearnesses, arose from the fact that these two methods were applied, actually, to successive and therefore sometimes divergent consciousnesses.

[21] See Dallenbach's criticism of Johnson, in this JOURNAL, 37, 1926, 153. See also, N. F. Gill and K. M. Dallenbach, A preliminary study of the range of attention, *ibid.*, 247-256.

This fact was shown in the introspective reports of Dallenbach's *Os*. One *O* declared: "The instructions ask too much for an adequate report, and any side of the report (naming, describing, number, order, form, etc.) could, it seems to me, be 'set for' without making the least difference in the processes which go on in the actual exposure period."

And later: "You know a lot about the colors after the exposure is over, but I am perfectly sure you lose a lot; as you start writing other aspects become non-reportable. It isn't so much the way you set yourself before the exposure that determines the report, but how you go to work after the exposure. I feel pretty sure that if *E* should call out after the exposure, 'Report on chroma,' I could report all chromas adequately, whereas usually by naming colors, mentioning clearness, brightness, etc., I lose certainty about chroma."

Another *O* said: "The colors persisted in the after-period, and at present I have the notion that the relative clearness in the after-period influences my report of the processes during the time of the exposure. That is to say, it is very difficult to draw a line, with everything else that I have to do, between these two periods."

The third *O* maintained: "What I report doesn't depend so much upon what I experience as upon where I start the report. When I concentrate upon the naming and description of the colors, my report on process is uncertain, and *vice versa*."[22]

TABLE IV

Degrees of Cognition	*Observers*						Totals for all *Os*	
	W		*F*		*D*			
	U	L	U	L	U	L	U	L
A 0-11 (15 rows)	5	324	2	63	10	196	17	583
B 12-17 (6 rows)	219	20	101	11	242	21	562	52

That the distribution of clearness in consciousness is subject to change, both during a short presentation-period and during the after-period, was definitely shown by results obtained in the present experiment with cases of alternations, in which a reversal of clearness-levels was found to coincide with the reversal of fields. It is not improbable, in those instances in which Dallenbach found the judgment of clearness and the data of the report to be inversely related, that some such comparable change of clearness-status had occurred.[23] One would not expect such redistribution of clearness to occur very often. Actually, in Dallenbach's experiment, it happened very infrequently. As is shown by Table IV, compiled from Dallenbach's data[24] there exists a high correlation between "levels of attributive clearness" and "degrees of cognition." Dallenbach's table shows for each *O* the number of color-square experiences

[22] *Op. cit.*, 198. Cf. also the correlations of upper level of clearness and order of naming, Dallenbach's Table IX.

[23] Rubin found figure and ground to be reversible in the after-image. Kats had obtained what was probably the same result in his experiments with the localisation of after-images. Rubin, *Visuell wahrgenommene Figuren*, 1921, 88-91; David Kats, Die Erscheinungsweisen der Farben, *Zsch. f. Psychol.*, Erg. Bd., 7, 1911, p. 59 esp.

[24] *Op. cit.*, 195, Table II.

that occurred for every combination of sensory clearness-status and cognitive clearness-status. The first column of his table gives stages of cognitive clearness from none to degree 17; Table IV is simply a condensation of Dallenbach's data in which two levels of sensory clearness were present. In row A is summed, for each *O* and for all *O*s taken together, the number of reports of experiences on the upper level of sensory clearness (U) and on the lower level of sensory clearness (L) in the first 15 rows of Dallenbach's table (cognitive steps 1-11 and 4 stages presumably below the level of cognitive clearness). In row B are the corresponding data for the last 6 rows of Dallenbach's table (cognitive degrees 12-17).[25] It is apparent from the table that most (97%) of the experiences judged low in 'sensory clearness' were relatively poorly described, and conversely that most (92%) of the experiences judged high in 'sensory clearness' were relatively well described.

Dallenbach's experiment, if interpreted on the basis here outlined, failed to show that clearness in human consciousness is to be conceived as of a peculiarly dual nature; the criticisms here advanced, if they are valid, leave the distinction of sensory and cognitive clearness without adequate experimental support. Dallenbach's experiment has shown merely that an *O* is capable of rendering a report upon the clearness of the various portions of his experience, and that when in a complex situation (like Dallenbach's) he sets out also to describe the entire content of his consciousness, changes may occur in the clearness-relations, so that what he ends with on paper will not correlate perfectly with that he may originally have included within his conscious range.

This correlation, one must add, while not perfect, is nevertheless high, and points therefore to some intimate connection of clearness and report. Here the results of the present experiment are in complete accord. The reports of the *O*s have been found to agree in showing that the outstanding thing about clearness is ability to report. This leads to the further suggestion that here, in the linking of clearness and description, is a criterion for clearness that is inherent, and, moreover, objective: the relative adequacy of the separate descriptive accounts which an *O* is able to render of the various components of a given consciousness may be used as an index of the clearness of these components. This measure of the clearness of the mental processes of an observer will hold reliably enough if it is employed for nothing more than it is a measure of, namely, immediate processes; if it is to be used as an index of the clearness-

[25]The separation between cognitive 'degrees' 11 and 12 is determined by the point of intersection—the same for all three *O*s—of the two slightly overlapping curves obtained by plotting cognitive degrees as abscissae and frequencies as ordinates for each level of sensory clearness.

relations of processes that are past, one must proceed with a caution commensurate with Dallenbach's data and the present data, which reveal that changes may occur in the clearness-relations for successive conscious experiences.

These criticisms of Dallenbach, if they are valid, leave the distinction of sensory and cognitive clearness without experimental foundation, but it would be incautious to go on to deny utterly the existence of any such differentiation; evidence in favor of a distinction may sometime be brought forward. It is difficult to prove the negative of such a question as this; one can hope merely to establish a probability in favor of one position or the other. Since, in the experience of figure and ground, an experience which has reason to be regarded as typical among perceptual phenomena, the differentiation has failed to appear even when the setting of the experiment was in its favor, it is apparent that the evidence of the present study does not support any distinction of sensory and cognitive clearness.

III. The Theory of the Two Levels

This review of clearness would not be complete without a consideration of the 'dual-level' hypothesis. This hypothesis was set forth in detail by Titchener in his *Feeling and Attention* in 1908.[26] Consciousness at any one time seems to show two levels of (sensory) clearness. These levels vary reciprocally; when the upper level is particularly clear the lower level is correspondingly obscure. When attention is less fixed the difference between the two levels is less marked. This duality, then, ranges between maximal and minimal difference. There are slight differences of clearness in the upper level, possibly also in the lower.

This theory Titchener modified slightly after Geissler's experiments.[27] Geissler, in an experiment on the measurement of attention in terms of degrees of clearness, found that the attentive consciousness is of two types, the dual-level and the multi-level types. This latter, rarer type manifests several (three or more) degrees of clearness in a given consciousness,[28] a conclusion which agreed with the earlier contention of Wirth that at times he had found more than two levels of clearness in his own consciousness.[29] Dallenbach then sought to do with auditory and with cutaneous presentation what Geissler had done with visual; none of his *O*s showed the multi-level type of consciousness.[30] In the course of the above-mentioned

[26] Pp. 220-242.
[27] *Experimental Psychology of the Thought Processes*, 293.
[28] L. R. Geissler, *op. cit.*, 527 f.
[29] W. Wirth, Zur Theorie des Bewusstseinsumfanges und seiner Messung, *Philos. Stud.*, 20, 1902, 493 f.
[30] Measurement of attention, this JOURNAL, 24, 1913, 506 f; Measurement of attention in the field of cutaneous sensation, *idem*, 27, 1916, 460. The 'law of the two levels' was further borne out in an experiment by N. F. Gill and K. M. Dallenbach, A preliminary study of the range of attention, *idem*, 37, 1926, 247-256.

experiment on attributive and cognitive clearness one of Dallenbach's *O*s reported multi-level types of consciousness, but it was later found that the levels were of cognitive, not of sensory clearness; and Dallenbach raised anew the question whether there are ever more than two degrees of clearness observable in the processes of a given consciousness.[31] He did not believe Geissler's evidence to have proved the possibility of a multi-level type.

The results of the present experiment are applicable to clearness in general and not particularly to sensory clearness, for indeed the experiment never revealed a clear-cut distinction between a sensory clearness and some other kind of clearness. The results do show that some perceptions occur with but two levels of clearness as clearness is here defined, and also that other perceptions occur with three, four, or even more levels of clearness. In the simplest, least equivocal case of figure-ground experience one field may be figure and the other ground; here typically the one field is of a high degree of clearness and the other of a low degree; here are but two levels of clearness. In the case of an intermediate experience, however, there may be, not two, but several fields in the perception, each on its own level of clearness; here is a multi-level condition. The presence of intermediates is a function of the ambiguity of the stimulus; when conditions are equivocal an intermediate perception is more likely to occur. It follows, therefore, that whether a given perception of figure and ground belongs to the dual-level or to the multi-level type depends at least in part upon the nature of the stimulus. If one argues from the figure-ground form of perception to perceptual experiences in general he will say that consciousness may contain at times two levels of clearness and at other times may contain more than two, and, moreover, that the equivocality of the conditions of the experience may play a part in determining the type of consciousness in a given case.

It may be possible to explain the variety of results that have been obtained in this problem of the two levels by pointing out, by analogy with the figure-ground phenomenon, that one would expect to find different conditions of stimulation, attitude, setting, etc., to vary in degree of equivocality and for this reason and perhaps other reasons also to give rise to perceptions sometimes of the dual-level type exclusively, at other times of both dual-level and multi-level types. If this is the case, agreement cannot be expected in experiments on the problem until the conditions are subjected to more adequate control.

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[31] Attributive *vs.* cognitive clearness, *op. cit.*, 226-7. In a footnote (p. 227) Dallenbach reviewed the case for the multi-level type of attentive consciousness.

Summary

In the perception of figure and ground attention plays a prominent rôle. The field that is figure is in general also the portion of the experience which stands forth as attentively clear, while the groundal field occupies a subordinate position in clearness. When conditions are equivocal, the experiences are frequently of an intermediate order. In this case the figure is made up neither of the one nor of the other stimulus-field exclusively, but of central portions of each field. The ground here, as indeed may be the rule, consists of peripheral portions of both fields. With such intermediates a close correlation is found to exist between the clearness-status and figure-properties of the several portions of the experience. If the two fields share about equally the property of figure, they are found also to occupy in most cases the same level of clearness. If one field predominates in the figural experience, its clearness is usually on the higher plane. A reversal of figure-ground fields may occur either during a presentation or in the after-image following the presentation; such an alternation of fields is accompanied by an exchange in clearness-status. Thus has been established a high, though not a perfect, correlation between figure-ground status and degree of clearness in a perception.

This clearness, it must be noted, does not appear to be either what has been called sensory clearness, or what has been termed cognitive clearness, for any differentiation of sensory and cognitive failed to be revealed. Whatever distinction Dallenbach found experimentally may have been a function of his setting. This clearness, moreover, does not seem to be limited by the dual-level hypothesis, for it may occur in consciousness not merely in two, but in three or four or many levels, according to the nature of the experience in question.

That attention in terms of the comparative clearness of the various portions of the experience is relevant to the problem of perception has been made apparent in the case of the figure-ground phenomenon by the demonstration of a correlation between a report upon clearness and a designation of the fundamental components of the perception. That this correlation has not been found to be perfect is but a point in favor of the view that an account of the phenomenon cannot be held to be complete if it does not include a description of the clearness-relations present in the experience. Thus a definite and objective formulation of the concept of attention comes into a very intimate and highly significant relation with a specific problem of perception, and points further to a generalization by which such a relationship between attention and the nature of the perceptual experience should be expected to obtain for psychological situations as a whole. Not only would such a generalization give a concrete and workable theory of attention, but it would lend itself immediately for application to the solution of the old and yet-remaining problem of descriptive adequacy in the field of perception.

THE BEST FITTING FREQUENCY FUNCTION FOR URBAN'S LIFTED-WEIGHT RESULTS

By H. M. Lufkin, University of Pennsylvania

Urban,[1] in assuming the $\Phi(\gamma)$-hypothesis, gives as the conditions which must be fulfilled (1) that the probabilities for a 'heavier' judgment must be expressed by a function f (x), which is asymptotic both to the x-axis and to the line y=1, or that

$$\lim_{x' = -\infty} f(x) = 0 \quad \text{and} \quad \lim_{x' = \infty} f(x) = 1$$

(for judgments of 'lighter' the limits are reversed); and (2) it is evident that this function must be expressed by means of an integral

$$y = \int_{-\infty}^{\infty} F(x)dx$$

which must exist and have a finite value greater than zero for all values of x from $-\infty$ to $+\infty$, and that the probability for any particular comparison stimulus shall be given by

$$p = \frac{1}{M} \int_{-\infty}^{x} F(x)dx.$$

There are many functions which would fulfill these conditions, but the one selected is

$$F(x) = e^{-G(x)}.$$

This function G(x) is expressed in the form

$$G(x) = h^2(x - a)^2$$

where x is the variable stimulus, a the limen, and h is a measure of precision. In this form

$$p = \frac{1}{M} \int_{-\infty}^{x} e^{-h^2(x-a)^2} dx$$

where $M = \int_{-\infty}^{\infty} e^{-h^2(x-a)^2} dx$. Urban shows that

$$M = \frac{\sqrt{\pi}}{h}.$$

*Accepted for publication April 7, 1927.

[1] F. M. Urban, Die psychophysischen Massmethoden als Grundlagen empirischer Messungen, *Arch. f. d. ges. Psychol.*, 15, 1909, 261-355; 16, 1910, 168-227.

This hypothesis has as its basic assumption that the probabilities arrange themselves in the form of the integrated probability function. If such a condition is true it will simplify the computations necessary to determine the constants which are desired. This can be done by the Method of Least Squares, but it must be remembered that this method is based on the assumption that the *arithmetical mean* is the most probable value that can be assumed for a series of observations.[2] From a series of observation equations, each of which is to be properly weighted,[3] the constants a and h can be obtained. The difference between the limens for observations of 'heavier' and 'lighter' is termed the 'interval of uncertainty,' in which neither the 'heavier' or 'lighter' judgments have a probability as great as 0.5. Urban[4] has found a rather distressing condition here, due to the fact that his probable errors for this interval are surprisingly large. This has led to the belief that the fundamental assumption of the $\Phi(\gamma)$-hypothesis is not valid and also that Urban's material, instead of being homogeneous, is heterogeneous. It is the purpose of this paper to determine these two points.

Since the publication of Urban's first paper a vast amount of work has been done in the field of statistical analysis, particularly in that branch devoted to 'frequency functions.' Karl Pearson[5] has developed his types of frequency functions, which are quite well known but as yet little used in psychometric work. There is also another type of function which is now coming into prominence, *i.e.* the Gram-Charlier. It is our purpose to examine Urban's original data and see if either of these functions are fundamentally better than the assumption of the 'probability integral.'

Since the frequencies of a judgment of 'heavier' or 'lighter' for any given comparison stimulus are in the form of an integrated function it is necessary to take the successive judgments for each value of the comparison stimulus. This gives us our values so that they can be handled as a frequency distribution. We may either work on the basis of the number of judgments or on the percentage of judgments. Whichever method is used, the results will be the same. Here the work has been done on the number of judgments, in order to simplify to some degree the work of curve fitting.

The use of the normal or Gaussian function necessitates only the computation of the first and second moments, that is,

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[2] L. D. Weld, *Theory of Errors and Least Squares*, 1921, 65-69.
[3] Urban, *loc. cit.*; W. Brown and G. H. Thompson, *Essentials of Mental Measurement*, 1921, 65-69.
[4] Urban, *loc. cit.*
[5] Karl Pearson, Skew variation in homogeneous material, *Phil. Trans. Roy. Soc.*, A, 186, 1895, 343 ff.; 197, 1901, 443-459; 216, 1916, 429-457.

the Arithmetic Mean and the Standard Deviation or σ. In the case of the Pearson and Gram-Charlier functions it is necessary to compute in addition to these the third and fourth moments about the mean, which is done in a manner analogous to the computation of the first and second moments. This raises at once a serious objection to the use of those two types of functions. Fisher[6] quoting Charlier says that it is generally useless to compute moments of a higher order than the second when the number of observations is less than 1000. Thiel gives the following rules:

For the first and second moments rely exclusively on the observed data.

For moments between the second and sixth rely partly upon theory and partly upon observations. These objections could

TABLE I

Constants for the Pearson Curves

O	Type	β_1	β_2	k_3	Range	Skewness	Mode
I H	J_I	1.052±.168	3.488±.220	−0.67±.06	82−106	−1.56	108.0
II H	I_I	0.452±.123	3.461±.342	−0.87±∞	36−112	−0.39	102.1
III H	I_I	0.735±.125	3.433±.260	−0.01±.16	76−109	−0.73	104.2
IV H	III(VI)	0.354±.164	3.566±.515	+4.13±∞	−116	−0.30	101.2
VI H	I_I	0.730±.185	3.618±.409	−0.07±.58	64−110	−0.60	103.7
VII H	I_I	0.711±.250	3.985±.717	−3.87±∞	−0−111	−0.44	102.9
I L	I_I	0.109±.038	2.656±.116	−0.08±.03	82−109	+0.25	90.3
II L	I_I	0.046±.020	2.465±.079	−0.00±.01	80−106	+0.17	93.1
III L	I_I	0.103±.052	3.153±.233	−26.4±∞	69−2630	+0.16	95.2
IV L	I_I	0.157±.071	3.019±.295	−0.28±.36	78−137	+0.23	92.9
V L	I_I	0.629±.166	3.550±.401	−0.70±.56	84−135	+0.52	90.4
VI L	I_I	0.010±.004	2.608±.113	−0.00±.00	76−117	+0.07	93.3
VII L	I_I	0.007±.004	2.658±.120	−0.01±.00	73−118	+0.06	93.1

be overcome in the case of the Gram-Charlier by the use of the method of Least Squares, but this would increase the amount of necessary computation to a very considerable extent.

Brown and Thompson[7] advise the use of Shepherd's correction for grouping, stating that the corrected moments are "probably still valid even without high contact, if the terminal frequencies are zero," but since high contact involves the vanishing of the first n derivatives, where n is large, this does not seem to be an advisable procedure in this case and the moments have been left uncorrected for grouping.

Table I shows the principal constants for the Pearson curves for thirteen of Urban's sets of observations.[8] The data of one O (V) were omitted for the 'heavier' since they gave negative frequencies when the differences of the judgments were taken. The determining factors in selecting the proper type of curve to fit the observations are the β's which are obtained from the

[6] Arne Fisher, *The Mathematical Theory of Probability*, 2nd ed. 1, 1923, 217.
[7] Brown and Thompson, *loc. cit.*
[8] For explanation of the symbols see W. P. Elderton, *Frequency Curves and Correlation*, 1906.

moments. These quantities are always positive. If $\beta_1 = 0$ and $\beta_2 = 3$, the Normal or Gaussian Curve is the best fitting, while if only $\beta_1 = 0$, the curve is symmetrical. The probable errors of the β's are of significance, for it will be noted that in all cases it would require more than twice the probable error of the β's to carry them over to zero, or the chance of a symmetric curve in any case is less than 4.5 to 1. The criterion k,

$$k = \frac{\beta_1(\beta_2 + 3)^2}{4(4\beta_2 - 3\beta_1)(2\beta_2 - 3\beta_1 - 6)},$$

is in all cases, except IV 'heavier,' negative, which means that a Type I Curve is the most probable. In the case of IV 'heavier,' k is greater than one and is finite, which would indicate Type VI, but in this case since the skewness, -0.30, is not large and $2\beta_2 - 3\beta_1 - 6$ is small, the curve fitted was Type III. This was done for purely practical reasons, since the data were of such a nature that in determining the maximum ordinate Gamma Functions[9] of large numbers entered into the computations if Type VI was used.

In four cases, II, IV, and VII 'heavier' and III 'lighter,' the probable errors of the criterion k approached infinity which indicated that the β's were of such a nature that they were near critical values where the types change. This is best seen from Chart I, which shows the location of the β's on about one-fourth of Pearson's Diagram.[10] The lines on this chart are obtained from the values of the β's which are typical for the various types of Pearson's curves. These curves are derived from a consideration of the point binomial $(p + q)^n$, where $p + q = 1$, which when expanded gives a series which represents the probabilities of the happenings of an event, $n_1(n-1)$ ....0 times when the probability of a single event is known; thus when $p = q$ we have a symmetric function which when n is large approaches the Gaussian Curve and when $p \neq q$ gives us a skewed curve, the first case being analogous to that of tossing a coin and the second that of tossing a die or a tetotem; but this does not represent all the probabilities that arise for there is another series which gives the chances of getting r, $r - 1$, ... 0 black balls from a bag containing pn black and qn white balls when r balls are drawn.[11] From differential equations derived from these series the critical values of the relationships between the β's are obtained which differentiate the types of curves and the lines on the chart are the graphs of these relationships. The point on the chart marked G repre-

[9] E. B. Wilson, *Advanced Calculus*, 1911, 378.
[10] Karl Pearson, *Tables for Statisticians and Biometricians*, 1914, lxii ff., 66.
[11] Elderton, *loc. cit.*, 36-46: Karl Pearson, Skew variation in homogeneous material, *loc. cit.*, 1895.

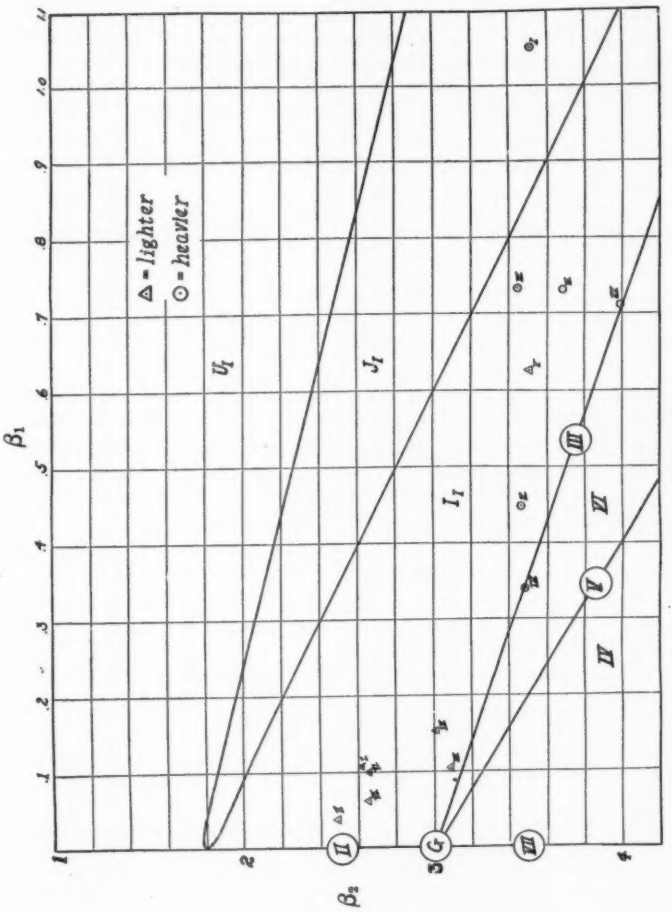

CHART I

sents the normal curve. When the point represented by the β's is very close to one of these lines the probable error of the criterion approaches infinity, *i.e.* IV 'heavier.' Quoting from a letter from Pearson in regard to this set of data, "Probably you would find it quite adequate to use a Type III curve for your data, but if you look at the chart on page 66 of the Book of Tables you will see that any one of the Types III, VI, or V, is likely to fit your data, and accordingly the probable error of the discriminating k becomes very large indeed." In this chart it will be noted that all the points are relatively close to the 'G,' *i.e.* Gaussian point, especially in the case of 'lighter' judgments.

As was to be expected all these curves are skewed, the 'heavier' in the negative direction and the 'lighter' in the positive direction.

The equation of the Type I curves is:

$$y = y_o \left(1 + \frac{x}{a_1}\right)^{m_1} \left(1 - \frac{x}{a_2}\right)^{m_2}, \text{ where } \frac{m_1}{m_2} = \frac{a_1}{a_2}.$$

In this case the origin is taken at the 'mode' and the curves are not asymptotic, but cut the x-axis at $-a_1$ and $+a_2$. The curve for I 'heavier' presents a peculiar form of Type I, where the curve starts at a finite point on the x-axis, rises rapidly, and has an infinite ordinate, but the area under the curve is finite. The equation of Type II is:

$$y = y_o e^{-rx} \left(1 + \frac{x}{a}\right)^{ra}$$

where the curve is asymptotic to the x-axis at one end but cuts at a fixed point at the other end.

The Gram-Charlier frequency function is given by the equation

$$y = N[\phi_0(x) + k_3\phi_3(x) + k_4\,\phi_4(x)],$$

where N = the number of observations, $\phi_0(x) = \frac{1}{\sigma\sqrt{2\pi}}\, e^{-\frac{1}{2}\left(\frac{x}{\sigma}\right)^2}$,

and $\phi_3(x)$ and $\phi_4(x)$ are the third and fourth derivatives of $\phi_0(x)$. In this case the origin is taken at the mean, and k_3 and k_4 are constants involving the third and fourth moments about the mean. These are called respectively the 'skewness' and the 'excess,' the first having a tendency to produce asymmetry, while the second tends to flatten the curve or to make it top-heavy.[12] In order to fit this curve it is necessary to have a

[12] Arne Fisher, *loc. cit.*, Parts II and III.

table giving the derivatives of $\phi_0(x)$, one of the best being that of N. R. Jørgensen in his Undersøgleser over Frequensflader og Korrelation,[13] which contains also other useful tables for curve fitting. Table II shows the values of k_3 and k_4 for the same data as the Pearson curves. It will be noted that these

TABLE II
Constants for the Gram-Charlier Curves

O	k_3	k_4
I H	+0.171	+0.020
II H	+0.118	+0.027
III H	+0.145	+0.019
IV H	+0.109	+0.023
VI H	+0.142	+0.026
VII H	+0.141	+0.041
I L	−0.056	−0.014
II L	−0.031	−0.013
III L	−0.052	+0.006
IV L	−0.062	+0.001
V L	−0.131	+0.028
VI L	−0.016	−0.016
VII L	−0.038	−0.013

TABLE III
Sum of the Squared Differences of the Observed and Theoretical Values Divided by the Theoretical

O	Normal	Pearson	Gram-Charlier
I H	60.7	24.8	65.1
II H	20.1	32.8	28.5
III H	69.9	17.7	25.6
IV H	75.7	76.3	45.5
VI H	20.2	6.1	4.0
VII H	21.9	10.9	13.3
I L	27.4	41.1	21.0
II L	9.0	25.1	6.0
III L	52.6	56.9	74.7
IV L	16.6	82.7	9.7
V L	69.8	45.0	63.5
VI L	14.9	30.6	8.6
VII L	32.5	21.1	27.0
Average	37.8	36.2	30.2

constants do not vary greatly in the various sets, an indication of homogeneity of the data.

Table III gives the values of the sum of the squared differences of the theoretical and actual values divided by the theoretical value for the Normal Curves, the Pearson Curves, and the Gram-Charlier Curves. There does not seem to be very much choice as to which type of curve gives the best fit. In

[13] N. R. Jørgensen, *Undersøgleser over Frequensflader og Korrelation*, 1916.

the case of the Normal curve, it is seen that twice it has the best fit, six times the second best and five times the poorest; while the Pearson Curves fit best five times, twice the second best, and six times the poorest. The Gram-Charlier Curves give the best fit six times, a second best fit five times, with a poorest fit only twice. If the higher moments had been adjusted by the Method of Least Squares the Gram-Charlier function would undoubtedly have given the best fit in all cases. The averages of the squared differences does not show a great amount of variation but the Gram-Charlier shows a superiority over the Pearson or the Normal forms.

A STUDY OF THE AUTOKINETIC SENSATION

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Introduction

In the study of the autokinetic sensation, we have to do with an illusion which was first noticed and reported by the astronomers. Von Humboldt[1] (1799) observed that a star appeared to move about when steadily fixated, and that the movement lasted several minutes. Schweizer[2] (1858) demonstrated that this movement was subjective by the simple fact that there was no agreement on the time and direction of the movement as reported simultaneously by several observers. He gave the illusion the name of *Sternschwanken*. He discovered at the same time that a black dot on a light background moved similarly—though through angles of smaller magnitude. The latter phenomenon became known as *Punktschwanken*.

Charpentier[3] (1886) made the first laboratory study of these movements. He found that a faint light in a dark-room wandered when fixated. De Parville[4] (1886) called attention to the similarity of the phenomena reported by Von Humboldt and Charpentier. Charpentier, however, objected to the comparison because he regarded the two phenomena as very different.[5] He held that the movements noted by Von Humboldt were oscillatory and of short excursion, whereas those he reported were uniform and steady and of long excursion.

The phenomenon reported by Charpentier was independently discovered by Aubert[6] (1887), while he was working in a dark-room on the perception of movement, and it was called by him the *autokinetische Empfindung*—by which name it has since been known. Since Aubert, special studies of these phenomena have been made by Exner,[7] Bourdon,[8] Carr,[9] Adams,[10] and Schilder.[11]

We became interested in the autokinetic sensations during the study of eye-movements in the fluctuation of attention.[12] Conditions were favorable in that study for the illusory movements, and as an apparatus for photographing eye-movements was already set up, we decided to test the eye-movement theories of the autokinetic sensation by photographing the

*Accepted for publication March 1, 1927.

[1] A. von Humboldt, *Kosmos*, 3, 1850, 73-74; *Voyage aux regions équinoctiaux*, 1, 125.
[2] G. Schweizer, *Ueber das Sternschwanken*, 1858.
[3] A. Charpentier, Sur une illusion visuelle, *Comptes rendus*, 102, 1886, 1155-1157.
[4] H. de Parville, Sur une illusion visuelle et l'oscillation apparente des étioles, *ibid.*, 1309.
[5] Charpentier, *ibid.*, 1462-1464.
[6] H. Aubert, Die Bewegungsempfindung, *Arch. f. d. ges. Physiol.*, 40, 1887, 459-480; Nachtrag su den Artikel, "Die Bewegungsempfindung," *ibid.*, 623-624.
[7] S. Exner, Über autokinetische Empfindungen, *Zsch. f. Psychol.*, 12, 1896, 313-330.
[8] B. Bourdon, *La Perception visuelle de l'Espace*, 1902, 333-342.
[9] H. A. Carr, The autokinetic sensation, *Psychol. Rev.*, 17, 1910, 42-75.
[10] H. F. Adams, Autokinetic sensations, *Psychol. Mon.*, 14, 1912, no. 59, 1-45.
[11] P. Schilder, Über autokinetische Empfindungen, *Arch. f. d. ges. Psychol.*, 25, 1912, 36-77.
[12] J. P. Guilford, 'Fluctuations of attention' with weak visual stimuli, this Journal, 38, 1927, 534-583.

movements of the eyes during the illusion. Before reporting the results of our photographs we first summarize the facts which must be accounted for by any theory, and then we review the theories and state the arguments that have been advanced both for and against them.

A summary of the results of the previous investigations reveals the following.

(*a*) *Conditions of movement.* The conditions favorable to the illusion are: (1) a dark-room; (2) a small stimulus-object; (3) a simple stimulus-object; (4) a stimulus-light of low intensity; (5) a steady fixation—sudden voluntary eye-movements destroy the illusion; and (6) fatigue—either general mental (Charpentier), or ocular (Bourdon).

(*b*) *Direction of movement.* The direction of the movement is reported as (1) a curved zigzag path which leads upward and outward (Charpentier), and return (Humboldt); (2) a circular or an elliptical path out toward the periphery and back again (Schweizer, Adams); (3) peripheral in 88% of the experiments regardless of the starting point (Adams), but the further the starting point is from the center of fixation the greater is this peripheral tendency (Carr); (4) in the direction of eye-strain in 26-91% of the cases—depending upon the distance of the starting point from the fovea (Adams); (5) dependent upon the preceding position of the eyes. If the eyes have just previously been directed to a point which is less than 30° from the fixation-point, the movement, upon fixation, will be toward that point. If the line of regard has been previously directed to a point which is more than 30° from the normal, the movement will start rapidly in the opposite direction, will decrease in speed and then return (Carr).

(*c*) *Rate of movement.* The rate of movement varies considerably among the observers. It is estimated as (1) 2°–3° per sec. (Charpentier); (2) 15°–20° per sec. (Carr); and (3) 10° or less per sec. (Adams).

(*d*) *Extent of movement.* The extent of the movement may be 30° or more (Charpentier); 20°–30° (Exner); 65°, under extreme fatigue (Carr); and 40°–50°, in the periphery (Adams).

(*e*) *Time of movement.* Movement begins, on the average, 6.1 sec. after fixation (Adams)—but the time varies considerably with the size of the stimulus-object.

(*f*) *Voluntary control of movement.* The movement is subject to voluntary control (1) in direction (Charpentier, Bourdon)—merely by the idea of a certain direction; (2) in duration, velocity, and extent (Carr); and (3) in form—it traces letters thought of by the observers (Adams). The voluntary control could not be verified by Aubert.

The first eye-movement theory of the autokinetic movements was proposed by Hoppe (1879). He came to the conclusion, in his treatment of the various kinds of illusory visual movement, that all such phenomena rest upon the functioning of the extrinsic eye-muscles.[13] The illusion, he thought, might arise either from the innervation of these muscles, or from actual involuntary eye-movements with the accompanying shifts of the image upon the retina.[14]

Charpentier (1886) tested the influence of gross eye-movements by fixating a group of dots, only the central (foveal) ones of which were visible. The group appeared to move in the usual manner without any change in the pattern of the dots. The dots were so arranged that if eye-movements had occurred the visible pattern would have changed. He eliminated abnormal tonicity of the eye-muscles as a condition of the

[13] J. I. Hoppe, *Die Scheinbewegung*, 1879, 84.
[14] *Ibid.*, 23.

phenomenon by a series of experiments in which he held his eyes in various positions in their sockets. No correlation was found between these abnormal positions and the occurrence of the apparent movement. He believed, however, that the brief oscillatory movements of the *Stern-* and *Punktschwanken* were due to slight displacements of the eye caused by unequal fatigue of the eye-muscles.[15]

Exner (1896) brought as evidence against the eye-movement theory the results of an experiment in which concentric fixation points were used—in the center of a dark disk, 1 cm. in diam., there was a bright pinhole. When these were fixated he found that the pinhole and the disk moved independently—the pinhole darted about within the disk, never reaching the boundary, and the disk moved in its own fashion.[16] Simon (1904) could not, however, verify Exner's results. He found that the pinhole and disk moved together.[17]

Bourdon (1902) showed that no gross displacements of the eyes occurred during the apparent movement. He let the spot move to an apparent position about 20° to the right, and then he turned on the light only to find that he was still fixating the stimulus-object. As a further test, when the spot had moved outward he brought his fixation back to the position which he thought was the starting point, and then turned on the light only to find that he was staring in a direction opposite to that of the apparent movement and displaced to about the same extent.[18] He believed, however, that the phenomenon was produced by an integrative effect of the slight but numerous movements of the eyes which were made in the direction of the apparent movement, and that these slight movements were brought about by the idea of that direction. When the outward movements of the eyes were slow, no movement of the stimulus-object occurred; but when the reflex backward movements were abrupt, an apparent displacement of the stimulus-object resulted.[19]

Carr (1910) distinguished three types of the illusion: (1) a type in which the fixation-point and stimulus-object moved together; (2) a type in which they moved independently; and (3) a mixed type in which they participated unequally—the fixation-point often lagged behind the stimulus-object.[20] He found, using various methods of testing for eye-movements, that there were no eye-movements during the illusions of type (1),[21] and that eye-movements were always present during the illusions of type (2) "whose direction, speed and extent always bore some definite relation to the corresponding characteristics of the illusion."[22] Carr, after an exhaustive study came to the conclusion that eye-movements played only a minor rôle, and that the effective factors were "(1) the position of the eye in its socket; (2) the after-effects of eye-position; (3) motor strains as exemplified in the tests on voluntary control; and (4) the after-effects of such strains."[23]

Schilder (1912) verified Exner's results with the pinhole and disk.[24] He also found that eye-movements, although they appear shortly after the beginning of fixation, do not in most cases coincide with the beginning of the apparent movement.[25]

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[15]Charpentier, *op. cit.*, 1462.
[16]Exner, *op. cit.*, 327.
[17]R. Simon, Über Fixation im Dämmerungsehen, *Zsch. f. Psychol.*, 36, 1904, 192.
[18]Bourdon, *op. cit.*, 339.
[19]*Ibid.*, 341.
[20]Carr, *op. cit.*, 43.
[21]*Idem.*
[22]*Ibid.*, 46. It is indeed strange that an involuntary eye-movement should produce a displacement of the stimulus-object and not be accompanied by a corresponding displacement of the fixation-point. There is also a discrepancy in his report of the extent of the movements of type (2). He says in one place (p. 43), "the average extent was approximately 10′′′ and in another (p. 60), "no movement greater than 1 cm. (16′) was observed."
[23]*Ibid.*, 69.
[24]Schilder, *op. cit.*, 52.
[25]*Ibid.*, 65.

From the results of these early studies, we were led to doubt whether eye-movements were the cause of the illusion. A theory of the autokinetic sensation based upon eye-movements alone can not account for all the facts that have been reported. The end result of the early investigations seems to be that eye-movements do not play an essential rôle. This is certainly true in the cases of gross illusory movement, and in the cases in which two stimulus-objects move independently; for, in the first instance, gross illusory movements are not paralleled by gross eye-movements, and in the second instance, independently moving stimulus-objects could not be, at one and the same time, produced by eye-movements.

The effectiveness of short involuntary eye-movements has not, however, been so clearly disproved. It may very well be, as Charpentier and Exner suggested, that the two kinds of movement—the short oscillations and the long excursions—are conditioned by different factors, and that eye-movements play an important rôle in the former, if not in the latter; or, as Bourdon suggested, that the large illusory excursion is a summation (under abnormal ocular circumstances) of small illusory movements which are conditioned by eye-movement.[26]

These questions of fact can not be settled until more accurate records, such as those given by the photographic method, have been made of the movements of the eyes during the illusion. It was in the hope of obtaining records which would incontrovertibly establish the relation between eye-movements and the autokinetic sensation that the present study was undertaken.

Apparatus and Procedure

Apparatus. The apparatus and technique which we employed in photographing the eye-movements were fundamentally the same as those of Dodge.[27] The slight variations from Dodge's technique are described in detail by the junior author in his study of eye-movements during the fluctuation of attention.[28] The principal variation consisted in the substitution of a moving film of a panoramic camera, after the method of Miles and Shen,[29] for Dodge's falling plate.

The stimulus, a circular red spot of light 2.5 mm. in diam., was 170 cm. distant, and in the direct line of regard. The room was completely dark except for the stimulus and the beam of light that was directed for photographic purposes upon the cornea. The corneal reflection was focused upon the film, and the light from a signal lamp within the camera box was adjusted to fall close beside it. (See Plate I.)

Procedure. The stimulus was exposed, in one series of experiments, to the left eye—the eye whose movements were photographed; and in a second series, to both eyes.

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[26]For logical proofs that apparent movement cannot be due to a nystagmus of small amplitude, see W. S. Hunter, The after-effect of visual motion, *Psychol. Rev.*, 21, 1914, 250-253.

[27]R. Dodge, An experimental study of visual fixation, *Psychol. Mon.*, 8, 1907, no. 35, 1-95.

[28]Guilford, *op. cit.*, 557-559.

[29]W. R. Miles and Eugene Shen, Photographic recording of eye-movements in reading Chinese, *J. Exper. Psychol.*, 8, 1925, 344-350.

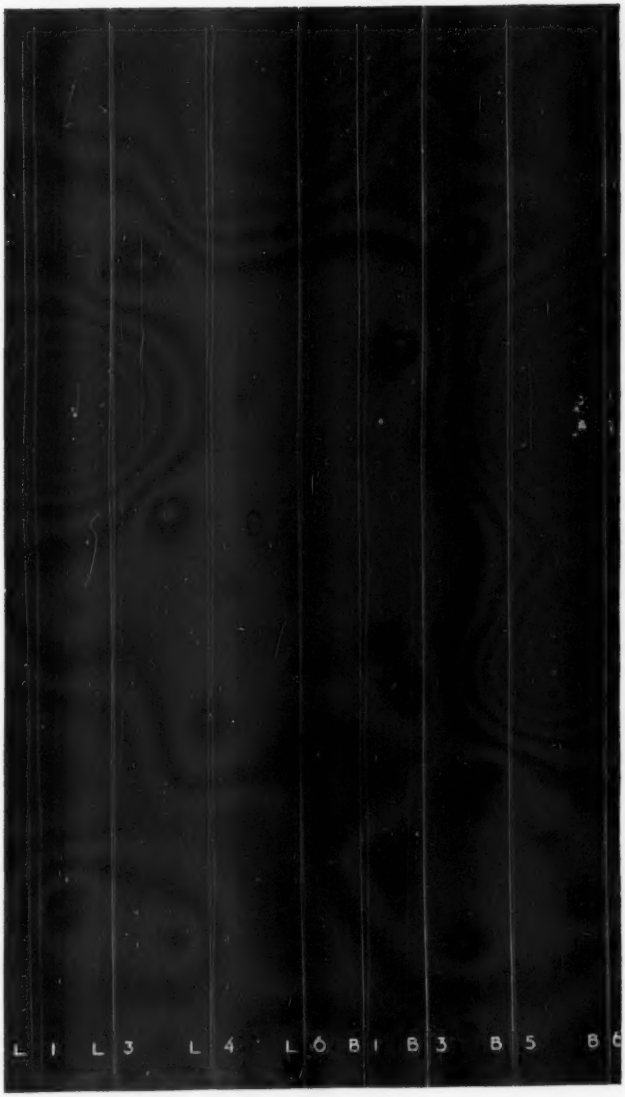

PLATE I

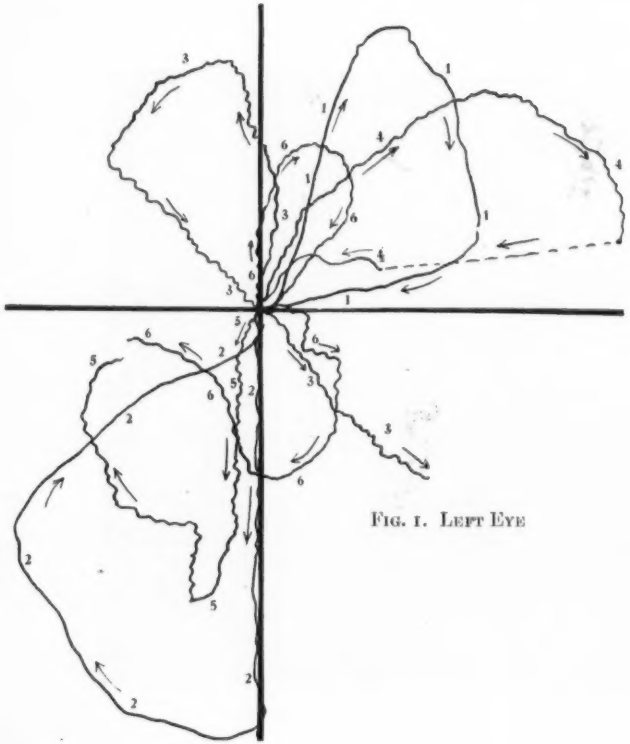

FIG. 1. LEFT EYE

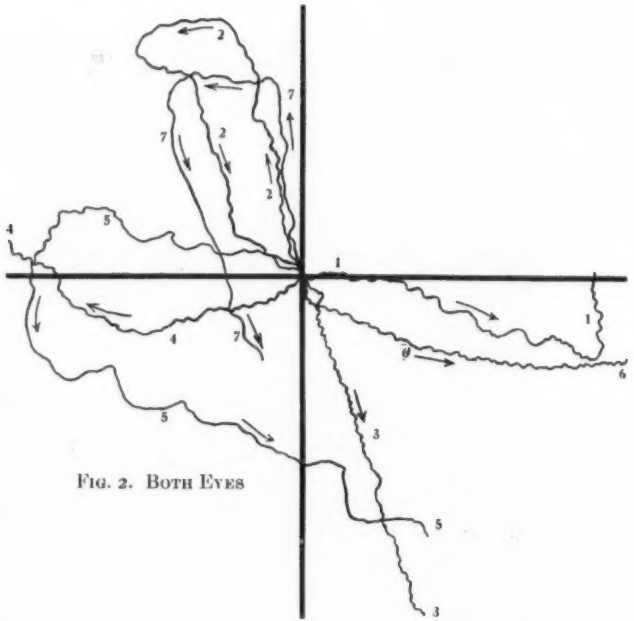

FIG. 2. BOTH EYES

The senior author (D) acted as the observer. He sat with arms resting on a table, and with head firmly held by a biting-board and a head-rest. *O* fixated the stimulus and signaled the appearance of the illusory movement by pushing a button that closed the circuit of the signal-lamp. When *E* saw the signal, he started the panoramic camera and allowed it to run either until *O* released the button as a signal that the movement had ceased or until the end of the film was reached. The signal light not only marked the beginning and end of the illusory movement, but it also gave a line on the film that served as a base of reference for the eye-movements.

At the close of the experiment and during its course, *O* made drawings of the path of the illusory movement. Six photographic records and drawings were made when the left eye alone was used, and seven were made when both eyes together were used. *O* also described at the end of the experiments any other phenomena that he noted during its course.

Results

The path of the illusory movement in the various experiments is shown in Figs. 1 and 2, and a typical section of the photographic record of the eye-movements is shown in Plate I. Records L_1, L_3, L_4, and L6 of Plate I show respectively the eye-movements during the first part of experiments 1, 3, 4, and 6 with the left eye; the path of the illusory movements in the same experiments is shown in Fig. 1. Records B_1, B_3, B_5, and B6 show similarly the eye-movements during the first part of those experiments with both eyes, whose illusory movements traced the paths shown in Fig. 2.

A detailed study of the photographic records shows that no gross movements of the eyes were made during the illusion; the eye-line paralleled very closely the base-line established by the stationary signal-lamp. As the photographic method was delicate enough to detect very small movements of the eyes—in special test-experiments it was found that a voluntary eye-movement of 1° displaced the eye-line approximately 0.7 mm.—eye-tremors were recorded. These movements are the only ones shown in the records; they are, however, too limited in extent to account for the gross illusory movements, and too irregular and too few in number to account for the small illusory movements, *i.e.* the oscillatory movements. We are forced, therefore, to the conclusion that eye-movements are not essential conditions of the phenomenon.

A positive and more significant result came from *O*'s reports. As the curves of Fig. 1 show, there is a remarkable uniformity in the pathways of the illusory movement. The typical path seems to be circular and along one of the diagonals of the visual field. Other investigators have described the paths as circular and elliptical, but no one has described the course more fully. Reports from other *O*s who noted the illusion in the experiments on the fluctuation of attention,[30] agree closely with those of D.

[30] Guilford, *op. cit.*, 582.

Since the course of the illusion seemed to be uniform, it occurred to us that the movement might be explained by the streaming phenomena,[31] which were frequently observed in this experiment. The movement of the streaming field might, we thought, be transferred to the fixation-point, just as in the case of the apparent movement of the moon behind floating clouds, the movement of the field is referred to the object. Evidence for such an explanation can be found also in comments made by another *O* during the previous experiments: "the stimulus seemed to be moving in a downward right direction behind a veil of some opaque material. It almost covered up the stimulus, and yet I had the impression that the stimulus was moving."[32] This *O* was undecided whether the stimulus was moving or the mists which obscured it.

It is significant that the patterns of the illusory movement reported by D, lie in the direction of the most frequent patterns of the streaming phenomenon which were found by Ferree,[33] and by Edridge-Green.[34] It is significant, too, that when both eyes are exposed to the light, the patterns become irregular, as shown in Fig. 2. We are told by Carr[35] and Adams[36] that the direction of the illusory movement varies for the two eyes, and that when the eyes are used together something like rivalry in direction occurs. We are told by Edridge-Green that the streaming phenomenon has a different direction in the two eyes—a circular direction to the right with the right eye, and to the left with the left eye;[37] and by Ferree that the streams are continually changing, with a few typical patterns. The streaming movement, in any case, would be different for the two eyes; hence the conflict in direction when both eyes are used, the direction being determined at any moment by the dominant eye.

Will the streaming phenomenon account for all the facts of this illusory movement? We believe it will. A small, simple stimulus-object in an otherwise blank field is most favorable to the illusion because the object then loses the anchorage of other objects in the field. The illusion is not apparent in a field full of objects; since the streams take various directions, they should exert a distorting or wrenching effect upon the objects; but the field, being comparatively stable, resists distortion; the streams are thus neglected, just as are other entoptic phenomena. In all similar illusions of movement it is the small, 'detached' ob-

[31] C. E. Ferree, The streaming phenomenon, this JOURNAL, 19, 1908, 484-503.
[32] Guilford, *op. cit.*, 581 f.
[33] Ferree, *op. cit.*, 491.
[34] F. W. Edridge-Green, Visual phenomena connected with the yellow spot, *J. of Physiol.*, 41, 1910, 263-275.
[35] Carr, *op. cit.*, 56.
[36] Adams, *op. cit.*, 19.
[37] Edridge-Green, *op. cit.*, 270.

ject which gives way to spatial displacement. A weak stimulus contributes to the illusion for the same reason—it is not a stable object.

Steady fixation is a favorable condition because it favors the loss of orientation in the visual field. Orientation is regained by a sudden voluntary eye-movement, and *O* often 'wakes up' to find the stimulus back at the starting point, although it had made no return excursion. An example of this is shown in Fig. 1; the broken line in 4 marks a shift in orientation without perception of the movement.

That the direction of movement is explained by the streaming phenomenon, we have already pointed out. The zigzag paths, *i.e.* the minor oscillations, have not, however, been accounted for. We have shown reason for doubting that eye-movements are a factor even of these smaller movements. They may be due to cross currents of the streaming phenomenon, but since the currents, according to Edridge-Green, have well-defined paths, this explanation does not seem likely. Pulse may have something to do with the oscillations, as on some occasions an oscillation of the stimulus was observed in about the same rhythm as the heart beat. The pulse in the retinal arterioles may have its immediate effect upon the currents, accelerating them for a moment in various directions.

The almost universal tendency for the illusion to go in a peripheral direction agrees with the observations of Edridge-Green that the streaming phenomenon flow toward the fovea. Of course there must also be outward movements of the retinal fluids, but none was observed by Edridge-Green. This means, at any rate, that the centripetal currents are more noticeable, and likewise that they have a primary effect upon the illusory movement. It is to be remembered in this connection, however, that Edridge-Green's theory of color vision demands such inward-flowing currents, and that Ferree described currents which extended through and beyond the fovea. In point of fact, a spot within the fovea could move, if our theory is true, only if the streams extended through and beyond the fovea.

The independent movement of two objects in the field, as observed by Carr, can easily be explained by the streaming phenomenon—the two objects have retinal positions in two different streams. Exner's pinhole and disk experiment is, however, a different matter (as they occupy identical retinal positions) unless one wishes to invoke cross currents, for which there is some evidence. We know that there are various other forms of entoptic phenomena besides the currents of Ferree and Edridge-Green, some of them of vascular origin. The pinhole, being so small, could suffer phenomenal displacement due to the movement of the blood streams in the retina.

That the movement is in the direction of eye-strain seems to offer no difficulty to our theory. It is as likely that the eye-strains are a result as a cause of the movement. The strains may arise under the difficult conditions of steady fixation. The illusory movement may be just as effective as a real movement in setting into motion innervation of the muscles for the eyes to follow the light; while inhibiting innervations, playing under the effect of instruction to keep fixation constant, prevent the eye-movements. We know of no evidence which will decide between this interpretation and that of Carr and Adams.

The effect of previous position of the eye is not so easily accounted for because we do not know all the conditions of the experiments in which this was found. Assuming that the observer brought the eyes to the fixation-point from the previous position abruptly and immediately observed the apparent movement, we may invoke the effect of eye-movement upon the retinal currents. Ferree found that voluntary eye-movements accelerate the currents in the retina in the direction of the eye-movement. Edridge-Green verifies this so far as the peripheral branches of the streams are concerned. We have, then, the conditions for the apparent movement which Carr reports in consequence of a previous position of the eye; the movement is along the line of eye-movement just executed.[38] The two cases, of apparent movement in one or both directions (in the direction of previous fixation or else away from it and return) cannot be safely interpreted unless more of the facts are known. It may be, however, that the currents are forced to flow in one direction for a time and then there is a rebound or backwash in the opposite direction.

The apparent rate of the movement is very significant for the streaming theory. The rate is much exaggerated as compared with the distance actually traveled by the stimulus-object. Whereas it moves from 10°-20° per sec., according to Carr and Adams, it rarely travels over 30° in all, and requires considerably more than 2 sec. to do so. This reminds one again of the moon which glides rapidly along but gets nowhere.

There has been some disagreement on the matter of voluntary control, but the consensus of opinion seems to be that one can vary the direction, velocity, extent, and duration of the movement, to some degree, at will. So far as velocity, extent, and duration are concerned, the control may come from sheer self-instruction; we see what we want to see or expect to see. The movement of the moon may be controlled in the same way. But this could hardly hold for the direction, if direction is conditioned by the streaming phenomenon which has its own

[38]Carr, *op. cit.*, 63-66.

direction. Is it conceivable that voluntary effort in some way determines the direction of these currents? This would be possible in that voluntary effort means the innervation of certain eye-muscles and in this way exerts certain stresses and strains upon the eyeball and hence puts pressure upon the retina at certain points. This becomes even plausible in view of Carr's results. He found that an ocular strain exerted voluntarily in any direction resulted in apparent movement in that direction and that the velocity could be controlled by the degree of muscular innervation. As an after-effect of strain in one direction the apparent movement went in the opposite direction and this after-effect lasted from 30-120 sec. Does release of strain allow the retinal currents to run back in the opposite direction?

All the facts can thus be explained by the streaming theory. Will observations bear out all our interpretations? We have reports from two observers to the effect that the field of vision about the moving stimulus-object seems to be moving in the opposite direction from that of the apparent movement. Our photographic records show that the eye-movements are not sufficiently large, numerous, or regular to account for the illusory movement. From our knowledge of other illusory movements we know that a small object yields to phenomenal displacement when its background is in motion. The patterns of movement observed by D coincide approximately with the pattern of the retinal currents found by others. We still need to determine the direction of the retinal currents for D, and perhaps to correlate the two kinds of patterns in a few other *O*s.

Loss of orientation in the dark is a well-known phenomenon and needs no further observation in this connection. The relation between the pulse and the oscillatory movements, however, must be more closely examined. The effects of previous positions of the eye upon the streaming processes should be determined. We should know what the currents are doing for at least 2 min. after the eyes are brought to fixation from previous positions. We should also test the effect of voluntary effort and strain exerted in different directions upon the direction of the retinal currents. Our theory stands or falls upon the results of these investigations.[39]

[39]This theory is closely related to similar theories for apparent movement of different types. Cf. Schilder, *op. cit.*, 70-71; A. H. Pierce, *Studies in Space Perception*, 1901, 331-338; and W. S. Hunter, Retinal factors in visual after-movement, *Psychol. Rev.*, 22, 1915, 484-486.

THE HISTORY OF THE EXPERIMENTAL STUDY OF THE KNEE-JERK

By Franklin Fearing, Ohio Wesleyan University

Perhaps no other reflex presents so variegated a history as does the knee-jerk.[1] Because of the ease with which it may be elicited, the objective definiteness of the response, the relative simplicity of the neural mechanisms involved, and its usefulness to clinical neurology, it has been almost a constant object of clinical and experimental investigation since its discovery in 1875. The extent of these investigations is indicated by the comprehensive monograph of Sternberg,[2] published in 1893, in which over 500 titles are listed. The history of these investigations and those of the succeeding twenty-five years is of especial interest to students of neuro-muscular phenomena since it parallels and illustrates certain major technical developments in the study of these phenomena, and offers data pertinent to their fundamental problems. These developments are concerned especially with methods of registration of muscular response, particularly as they relate to the technique of the study of reflex temporal relationships, and to the problems of inhibition and facilitation.

Wilhelm Heinrich Erb (1840-1921) and Carl Friedrich Otto Westphal (1833-1890) independently described, in 1875, the phenomenon which occur when the patellar tendon is stimulated. In Erb's paper[3] the phenomenon is referred to as the "Patellarsehnenreflex," while Westphal[4] designated it as the "Unterschenkelphänomen." Grainger and Gowers appear to have been among the first of the English neurologists, and Charcot and his students among the first French neurologists, to investigate the phenomenon. In addition to the designations of Erb and Westphal, the following are some of the English terms used in referring to the reflex: "tendon phenomenon," "tendon reflex," "knee-jerk," "knee-reflex," "tendon jerk," and "patellar reflex."

Erb and Westphal immediately disagreed as to the essential nature of the newly discovered phenomenon. The former

[*]Accepted for publication March 31, 1927.

[1]This study was begun at Stanford University.

[2]M. Sternberg, *Die Sehnenreflexe und ihre Bedeutung für die Pathologie des Menschen*, 1893.

[3]W. Erb, *Ueber Sehnenreflexe bei Gesunden und Rückenmarkskranken;* cf. O. Westphal, *Arch. f. Psychiat. u. Nervenkr.*, 5, 1875, 792.

[4]O. Westphal, Ueber einige Bewegungserscheinungen an gelähmten Gliedern, *Arch. f. Psychiat. u. Nervenkr.*, 5, 1875, 803-834.

regarded the muscular contraction in response to the stroke on the patellar tendon as a 'true' reflex. In opposition to this Westphal regarded the response as being the direct mechanical response of the muscle to the stroke on its tendon. For Westphal a state of reflexly maintained tonus was essential for the appearance of the muscular response to the stroke on the patellar tendon. Brondgeest[5] in 1860 performed the fundamental experiment in which it was demonstrated that when a spinal frog is suspended the legs hang in a more relaxed condition if the afferent roots of the sciatic plexus have been cut, than when the nerve supply is intact. Johannes Müller and Marshall Hall had suggested previously that the muscles were always in a state of tension which was dependent upon a nervous connection. Gowers, Waller, and others suggested that a reflexly maintained muscular tension is essential to the appearance of the jerk.

It was urged against the reflex theory (1) that the time of the knee-jerk, *i.e.* the time between the stroke on the tendon and the beginning of the muscular contraction, is too brief to allow for conduction to and from the spinal cord; (2) that the knee-jerk does not show the characteristic 'purposiveness' of 'true' reflexes; (3) that the knee-jerk is not inhibited, but only affected through the action of antagonists; and (4) the knee-jerk is not affected by narcotic agencies.[6] Of these four, the major discussion centered about the time-relationships involved in (1). To understand this discussion it is necessary to review the interesting development of experimental techniques in the study of neuro-muscular phenomena—a development which is of significance for the understanding of present-day investigations in this field.

Development of Experimental Techniques in the Study of Tendon Phenomena

The experimental techniques in connection with the study of the tendon responses have undergone an interesting metamorphosis. The method of Erb and Westphal—and the method used today in the neurological clinic—was simple; the subject was required to sit either with the leg swinging freely, or with the legs crossed, and the operator struck the patella a smart blow either with the side of the hand or with a percussion hammer. The height and rapidity of the resulting upward kick of the leg was noted by the operator. In the experimental studies in-

[5] P. J. Brondgeest, *Arch. f. Anat. u. Physiol.*, 1860, 703-704. A summary of the work, both early and recent, on tonus may be found in J. F. Fulton, *Muscular Contraction and the Reflex Control of Movement*, 383 ff.

[6] See Sternberg, *op. cit.*, 55.

strumental aids became necessary in recording the contraction, and in standardizing the stimulus to be applied to the tendon.

In recording the knee-jerk, three indicators (to use the phrase of Dodge),[7] may be listed in the order of their historical development: (1) thickening of the muscles in the stimulated leg; (2) movement of the leg directly recorded; and (3) registration of the action current in the muscles by the galvanometric technique. Brissaud in 1880, ter Meulen, Jarisch and Schiff in 1882, and others used the registration of the thickening of the muscle as the indicator of the response.[8] Bowditch and Warren,[9] Lombard,[10] and others recorded directly the movement of the leg. The registration of the action current in the stimulated muscle by the galvanometric technique made possible the exact study of the time relationships. Among the first to make use of this technique in the study of knee-jerk was Dodge. Jolly and Snyder in 1910, Hoffman in 1922, and Golla and Hetwer in 1922 are among those who have used the string galvanometer. The direct registration of the movement of the leg may be accomplished by a system of levers and pulleys, which actuate electrical or pneumographic time-markers. Dodge, in his monograph, found this method the least satisfactory. Bowditch and Warren, however, made use of this technique in a study, the results of which are quoted in nearly every modern physiological text. The method of recording the thickening of the muscle has been adopted in many of the recent investigations, *e.g.* Miles,[11] and Dodge and Benedict.[12] It is interesting to note, however, that Tuttle[13] in a series of investigations in 1924 recorded the excursion of the leg by means of a lever system. In recording directly the thickening of the muscle, it is possible to obtain a record of the moment of stimulation of the tendon, since the blow of the stimulating instrument causes a slight mechanical twitch in the muscle which precedes the reflex response proper and is recorded on the kymographic records. This feature is an important advantage of this method.

The registration of the response has for its object the demonstration of one or more of the following aspects of the jerk: (1)

[7] R. Dodge, A systematic exploration of a normal knee-jerk, its technique, the form of muscle contraction, its amplitude, its latent time and its theory, *Zsch. f. allg. Physiol.*, 12, 1911, 1-58.

[8] See Sternberg, *op. cit.*, for a review of this work.

[9] H. P. Bowditch and J. W. Warren, The knee-jerk and its physiological modification, *J. Physiol.*, 11, 1890, 25-64.

[10] W. P. Lombard, The variations of the normal knee-jerk and their relation to the activity of the central nervous system, this JOURNAL, 1, 1888, 8.

[11] W. R. Miles, Alcohol and human efficiency, *Carnegie Inst., Pub.*, 1924, No. 333.

[12] R. Dodge and F. S. Benedict, Psychological effects of alcohol: an experimental investigation of the effects of moderate doses of ethyl alcohol on a related group of neuromuscular processes in man, *Carnegie Inst., Publ.*, 1915, No. 232.

[13] W. W. Tuttle, Apparatus for eliciting and recording the patellar tendon reflex, *Amer. J. Physiol.*, 68, 1924, 338.

the time between the stimulus and the beginning of the contraction—the reflex time; (2) the form (height, steepness of ascent, etc.) of the contraction curve; and (3) the duration of the contraction. The various methods of registration described emphasize one or more of these factors.

It is essential that the form of stimulation be standardized. A percussion hammer is usually used and may be actuated by a coiled spring or electrically released from above the knee and actuated by gravity. It is probable that the latter method secures the most satisfactory mechanical control.

Temporal Relationships in the Tendon Phenomenon

In 1875 the discussion regarding the reflex nature of the newly discovered tendon phenomenon chiefly centered about the time relationships. It was asserted by those who adhered to Westphal's point of view that the jerk was not a true reflex, *i.e.* did not involve neural conduction to and from the spinal cord, because the time was too brief to permit such transit. Foster, in 1897, some twenty years after the discovery by Erb and Westphal, gives perhaps the best statement of this view.

"If the time which elapses between the blow on the tendon and the beginning of the movement be measured, this is found to be very short, .02 sec., far shorter than that of any known reflex act; and indeed if the measurement be made between the tap and the very beginning of the contraction of the muscular fibres themselves (as may be done by a special arrangement), the interval is actually less than that which occurs when the contraction is brought about by stimulating the motor nerve at some distance from the muscle. It seems impossible, in view of this fact, to regard the act as a reflex act; and we are led to the conclusion that the contraction is idiomuscular, is brought about by the vibrations started in the tendon running up to the muscle and directly stimulating the muscular fibres, and not by nervous impulses making the reflex circuit through the spinal cord."[14]

Foster proceeds to state, however, that the irritability of the muscle and its readiness to respond to direct stimulation is dependent upon the spinal cord, *i.e.* the muscle is in a state of tonus which is maintained by a nervous arc involving the cord.[15]

[14] M. Foster, *A Textbook of Physiology, Part III: The Central Nervous System*, 7th ed., 1897, 999.

[15] Cf. C. S. Sherrington, Note on the knee-jerk and the correlation of action of antagonistic muscles, *Proc. Roy. Soc.*, 52, 1893, 556-564. In this paper Sherrington demonstrated that the knee-jerk could be *inhibited centrally like other spinal reflexes*. He says: "It would thus seem clear that the exaggeration of the knee-jerk produced by severance of the branches given from the great sciatic nerve to the hamstring muscles is not due to the fact that the resulting relaxation of those muscles simply leaves the joint mechanically more free to move. The exaggeration would seem due rather to the severance of the nerves in question interrupting a stream of centripetal impulses that passes up from the hamstring muscles and enters the spinal cord by certain afferent roots, and in the cord exerts a depressing or restraining influence on the jerk . . . . . Further, it would seem that at the knee joint excitation of the afferent fibres coming from one set of the antagonistic muscles induces reflex tonic contraction of the opposing set with extreme facility, despite the fact that the opponent muscles are not innervated from the same spinal segments . . . . Thus the degree of tension in one muscle of an antagonistic couple intimately affects the degree of 'tonus' in its opponent, not only mechanically, but also reflexly, through afferent-efferent channels and the spinal cord" (p. 563). This is strong evidence in favor of the reflex nature of the jerk.

In the period from 1875 until 1895 a number of investigators measured the time of the tendon phenomenon, that is, the time elapsing between a blow on the patellar tendon and the beginning of the muscular contraction. Table I summarizes these findings.[16]

TABLE I

Investigator	*Date*	*Time* (sigma)
Tschirjew	1879	58–61
Gowers	1879	15–90
Brissaud	1880	50
ter Meulen		35–90
Waller	1880	30–40
James	1880	25
Eulenberg	1882	24
De Watteville	1882	20
Rosenheim	1884	31–33
Jendrassik	1894	23

Exner[17] in 1874 had measured the time of the wink-reflex in response to an electrical stimulus, obtaining values between 57 and 66 sigma. From these times Exner subtracted the conduction time in the nerve and the time of the muscle latency. He thus obtained the 'reduced reflex time' between 47 and 55 sigma.

Although there were great differences in the times obtained by Tschirjew, Gowers and others in measuring the tendon response—due in part, of course, to differences in methods used—it is clear that these times are of an order of magnitude in general much below that obtained by Exner in measuring a 'true' reflex. Waller, in discussing the times that he obtained, states in 1890, that in man any reflex movement of the leg must take 'at least' 100 sigma. Exner's are less than this, but Waller says:

> Moreover it may be urged that we do know that Exner's reflex, viz., the closure of eyelids consequent upon stimulation of the conjunctiva, which is the nearest approach we know in man to a pure reflex, is only .05 second. This item does not, however, seem to me to incline the balance in either direction; it is the shortest known 'reflex time,' and it is the reaction of a very mobile muscle of small bulk, via very short channels of innervation. Admitting the measurement (and I have verified it) I do not think that it weighs either way; it is longer than the 'direct' intervals quoted above, but not sufficiently so to contrast with them; it is probably shorter than a true reflex time of the leg or thigh but not sufficiently short to allow us to admit a rectus femoris reflex time of only .03 second. In sum it cannot be used as evidence in either sense.[18]

[16] Cf. Nagel, *Handbuch*, IV, 1909; Sternberg, *op. cit.*, 29 ff.; E. Zeising, *Ueber das Kniephänomen mit specieller Berücksichtigung des normalen und pathologischen Verhaltens desselben im Kindesalter*, 1887, 15 ff.; H. Netter, *Zur Geschichte der Lehre vom Kniephänomen bei Geisteskranken nebst Beobachtungen über diesen Gegenstand aus Freiburger Psychiatrischer Klinik*, 1897; T. Rosenheim, Experimentelle Untersuchung der unter dem Namen "Sehnenphänomene" bekannte Erscheinungen unter möglichster Berücksichtigung von Versuchen an Menschen und pathologischen Beobachtungen, *Arch. f. Psychiat. u. Nervenkr.*, 151, 1884, 184-201.

[17] S. Exner, Experimentelle Untersuchung der einfachsten psychischen Processe, *Pflüger's Arch.*, 7, 1873, 601-660; 8, 1874, 526-537.

[18] A. Waller, On the physiological mechanisms of the phenomenon termed tendon reflex, *J. Physiol.*, 11, 1890, 387 f.

Waller believed, then, that the tendon phenomenon was not a true reflex, but that the integrity of the reflex arc was a *sine qua non* condition. Data obtained from experimentation on rabbits confirmed these results. Waller found that the times for the tendon phenomenon in the rabbit were about 8 sigma, while 'true' reflex contractions, obtained by stimulating the skin of the same animal, were found to take place in about 33 sigma.

Accurate timing of reflex phenomena was difficult with the techniques used by these investigators. After pointing out the need of more accurate timing methods, Sternberg indicated that positive experimental proof of the nature of the tendon phenomenon was not available, since too much weight was not to be attached to the short latencies.

Es ist daher dem Argumente der zu kurzen Reflexzeit nur ein geringes Gewicht beizumessen. Man muss sich sagen, dass wir uns eben mit der Thatsache befreunden müssen, dass so einfache Reflexe, wie die Sehnenreflexe, bei denen keine Coordination von Muskeln in bestimmter Reihenfolge nöthig ist, wie das etwa beim Zurückziehen eines Beines der Fall ist, in ganz kurzer Zeit verlaufen können. Auch ist eine so kurze Reflexzeit für den wahrscheinlichen Zweck der Sehnenreflexe erforderlich.

Wiewohl also ein exacter experimenteller Beweis nicht gegeben ist und höchstwahrscheinlich überhaupt nicht gegeben werden kann, spricht die weitaus grössere Wahrscheinlichkeit dafür dass das Muskelphänomen beim sogenannten Sehnenreflexe in gleicher Weise wie das Knochenphänomen als ein Reflexvorgang aufzufassen ist.[19]

With the use of the galvanometric technique with photographic record of the thread deflections by Jolly[20] and Snyder[21] in 1910 and 1911, the fact was demonstrated that the time between stimulus and response in the case of the knee-jerk is theoretically long enough to allow for conduction to and from the spinal cord. Dodge had already demonstrated that the time of the responses of the muscle to direct stimulation is shorter than the times obtained for the knee-jerk by the earlier investigators. He was able to obtain times in the neighborhood of 15 sigma in the case of the extirpated frog's muscle. Jolly in 1911, using the galvanometric technique, obtained times in the neighborhood of 6 sigma for the knee-jerk in the spinal cat. In the case of man the same observer found the average of 14 observations on 3 subjects to be 20.5 sigma, 22.7 sigma, and 20.7 sigma respectively. Snyder found an average time of 11.3 sigma in man. Jolly pointed out that "it is, therefore, necessary to reconsider the question as to whether the latent period of the knee-jerk is long enough to include the various times occupied in reflex action."

[19]Sternberg, *op. cit.*, 56.

[20]W. A. Jolly, The time relations of the knee-jerk and simple reflexes, *J. Exper. Physiol.*, 4, 1911, 67-88; Reflex times in the South African clawed frog, *Proc. Roy. Soc.*, 92B, 1921, 31-51.

[21]C. D. Snyder, The latency of knee-jerk response in man as measured by the thread galvanometer, *Amer. J. Physiol.*, 26, 1910, 475-482.

These 'times' are (1) latency of the afferent endings; (2) time of conduction from the vastus internus and part of the crureus divisions of the quadriceps to the cord by the anterior crural nerve, and back by the same nerve-trunk to the muscle; (3) latent period of the electrical change in the muscle; and (4) time occupied at the synapse.

Analyzed on this basis, the total time of the response in the spinal cat yields the following, according to Jolly.

Total latency of jerk		6.6
Latency of afferent endings	.97	
Nerve conduction	1.45	
Latency of motor endings	2.38	
	4.80	4.8
Synapse time		1.8

It is notable that the times found for man by Jolly were very similar to those offered by Waller as proof that the tendon-phenomenon was not a true reflex. It must be pointed out that the distribution of times presented above represents the situation only under a certain set of experimental conditions.

In 1921, Jolly studied the times for homonymous and heteronymous reflexes in the frog, with the view of finding the differences between the two types and thus "to obtain a measure of the delay in transmission of the reflex impulses in the spinal cord, or 'synapse time.'" Spinal and decerebrate animals were used and the time measurements were obtained by means of the Einthoven string galvanometer. The average time for 41 cases of the heteronymous reflex was 15.7 sigma, while the average for 40 cases of the homonymous reflex was 14.3 sigma. Regarding the difference Jolly says:

> I do not think that the above experiments taken by themselves would justify us in assuming that in the spinal frog at the temperatures employed, and with the frogs in good condition, the intraspinal path of the heteronymous reflex involves a larger number of synapses than that of the homonymous reflex. . . . . It would rather appear that we are dealing with mechanisms involving the same number of neurones on the two sides. . . . The fact that the homonymous time is usually slightly the shorter may possibly be due to the motor neurones responding rather more readily to stimuli reaching them through afferent nerves from the same side of the body.[22]

Jolly analyzed the total time as follows:

> If we select a reflex time recorded at a room temperature of 23 degrees C. from a frog in good condition, first response, thirty-five days after operation, we find an interval of 11.4 sigma for the total reflex time. The length of nerve from toes to cord and from cord to thigh was found post mortem in this frog to be 16 cm. The rate of conduction in the nerve of Zenopus at 23 degrees C. was determined in the usual manner by electrical stimulation at two points, with the electrical change in the muscle as in-

[22] Jolly, *op. cit.*, 35.

dicator, and was found to be 37 m. a second. The latency of the muscular response to stimulation close to the muscle was found to be 2.4 sigma. If we allow one sigma for the latency of sensory nerve endings we may suggest the following analysis:[23]

Total reflex time		11.4
Nerve conduction	4.3	
Muscle latency	2.4	
Latency of sensory endings	1.0	
	7.7	7.7
Synapse time		3.7

The same type of temporal analysis is exemplified in the work of Hoffman[24] who has studied the tendon-reflex (which he terms "Eigenreflexe") and in his monograph of 1922, he characterized these temporal elements as follows:

L = the conduction time in the nerve (*Leitungszeit*)
I = the length of the nerve
N = the rate of conduction (*Leitungsgeschwindigkeit*)
M = the latency of the muscle
S = the latency of the end-organ
R = the 'true' reflex time

The total reflex time ('Z'), then, is equal to R + M + L + S. Hoffman emphasized the importance of determining the value of N (the rate of conduction). This value varies, *i.e.* it is dependent upon the *length of nerve*. The relationship is one in which the rate decreases as the length of the nerve increases. Hoffmann arrived at this conclusion by dividing the length of nerve expressed in cm. by the total reflex time (I/Z).

Je grösser der Nervenweg ist, der beim Reflex durchlaufen werden muss, um so geringer ist die Brutto-geschwindigkeit des Prozesses I/Z. Mit anderen Worten, wenn man von Muskellatenz, Latenz der sensiblen Endorgane, und reiner Reflexzeit ganz absieht, so ist die Leitungsgeschwindigkeit um so langsamer, je grösser die Nervenstrecke ist, die durchlaufen werden muss. Dies gilt für alle Möglichkeiten, den Eigenreflex zu messen, mit Ausnahme des Masseterreflexes. Hier ist die Nervenleitung so kurz, dass die Muskellatenz und die reine Reflexzeit schon entscheidend ins Gewicht fallen.[25]

In comparing the gross velocities (I/Z) in 5 cases with a short reflex arc (170 cm.) with those having a long reflex arc (190 cm.), he found the velocity in the first to be 56.7 m. per sec., and in the second 49.3 m. per sec.

The total time, according to Hoffmann, for any reflex remains relatively constant in the normal individual; in the diseased

[23] *Op. cit.*, 37.

[24] P. Hoffmann, *Untersuchungen über die Eigenreflexe (Sehnenreflexe) menschlicher Muskeln*, 1922. Hoffmann points out that the tendon plays no essential part in connection with the release of the reflex; the end-organs lie within the muscle itself. Every muscle has an *Eigenreflex* which, according to him, is purposeless and isolated from other types of reflex activity.

[25] *Op. cit.*, 47.

individual this time is lengthened. The rate of conduction in the case of disease is slower than in the case of health. The following table indicates the values of I/Z for the Achilles tendon reflex in certain diseased conditions.

In internal diseases (inneren Krankheiten)	50.1 m. per sec.
Organic disease of the central nervous sytem	51.5 " " "
Other neuroses	49.6 " " "
Functional nervous diseases	59.9 " " "

Hoffmann regards the tendon-reflex as occupying a peculiar position in that there is only one synapse involved in the spinal cord. The time occupied in the cord is about one sigma.

The analyses of Hoffmann, Jolly, Snyder, and other contemporary investigators of the time relationships of the knee-jerk, tell a dramatic tale of progress in experimental technique. Johannes Müller in the middle of the 19th century had prophesied that the velocity of the nervous impulse would never be measured, but lived to see his student, Helmholtz, measure it in 1850. The measurements in 1879 and 1880 of the time of the knee-jerk made possible only comparatively rough approximations of the duration of the various part processes composing the reflex.

Waller[25] in 1890 summarized under seven heads the evidence bearing on the reflex nature of the tendon phenomenon. These are (1) lost time of the tendon-phenomenon as compared with the lost time of direct and of reflex muscular contractions; (2) lost time of tendon-phenomenon, in muscles at different distances from the cord; (3) clinical observations; (4) experimental section of nerves and of nerve roots; (5) destruction of the spinal cord; (6) muscular tonus; and (7) crossed tendon-reflexes. The evidence from all of these fields of investigation except (1) and (2) indicate that a reflex arc is essential for the knee-jerk. In the opinion of Waller the evidence from the measurement of the time relations tends to indicate that the time is too brief for a reflex to take place, and hence the phenomena are not reflex, but are dependent on the reflexly maintained muscular tonus. The data which have been presented by Dodge, Jolly, Snyder, Hoffmann, and others, seem to indicate that this objection of Waller's has yielded before improved methods of temporal measurement in physiology and psychology.

Dodge in 1910 presented evidence which tended to establish further the reflex nature of the knee-jerk. This evidence may be summarized: (1) the course of the wave of muscular contraction was found to proceed from the central end of the muscle rather then from the distal end, as might be expected if the contraction were the result of direct mechanical stimulation;

[25]Waller, *op. cit.*

(2) the length of the reflex arc involved increases the total time occupied by the reflex, *e.g.* the latent time of the knee-jerk is from 11 to 18 sigma; shorter than that of the Achilles tendon response; (3) the knee-jerk may take place only as a result of coördination of flexor and extensor muscles; a nerve center is essential to such coördination, and hence a reflex arc is implied.[27]

Variability in Tendon Reflexes

The enormous variability of the tendon-reflexes has been a subject of comment by practically every investigator of these phenomena. It was usual in 1875—as it still is in many quarters—to regard the reflex as a stable and relatively unmodifiable reaction. The possibility that the course of the reflex is modified by simultaneously acting neuro-muscular mechanisms suggested itself as an explanation of the observed variability. The phenomena of facilitation and inhibition point to the existence of such functionally coöperative systems. *Bahnung* and *Hemmung* as objects of scientific investigation had antedated, of course, the discovery of the knee-jerk.[28]

The study of the *cortical influences*[29] as factors in the variability of the knee-jerk is perhaps the most significant advance from the point of view of the development of reflex theory.

Jendrassik[30] in 1885 noted the differences in the percentages of failure to elicit the knee-jerk as reported by different investigators. Berger had reported 1.56% failure among his subjects, Eulenberg 4.8%, Block .72%, Pelizaeus .04%. Jendrassik suggested that these differences might be due to the techniques used by the investigators, and suggested that if some other muscle groups were thrown into contraction *simultaneously with the knee-jerk*, the latter would be exaggerated.

> Zu diesem Zweck benutzte ich meine Erfahrung, nach welcher die Sehnenreflexe, besonders aber das Kniephänomen sich bedeutend steigern, wenn wir mit den übrigen Muskeln unseres Körpers grosse Kraft erzeugen. So, z. B., fällt das Kniephänomen viel stärker aus, wenn wir die Muskeln der Arme stark anspannen. Ich setze das betreffende Individuum, welches mit der gewöhnlichen Methode kein Kniephänomen hatte, auf den Rand eines Tisches mit möglichst erschlafften Beinen, and während ich auf seine Patellarsehne klopfe, fordere ich es auf, die gebeugten Finger der rechten und linken Hände in einander auszuhängen und sie bei nach vorne ausgestreckten Armen so stark als möglich auseinander zu ziehen.[31]

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[27] Cf. Sherrington, *op. cit.*

[28] Cf. W. H. Howell (Inhibition, *Physiol. Rev.*, 5, 1925, 161-181) for a recent review of the theories of inhibition. The monograph by J. F. Fulton (*Muscular Contraction and the Reflex Control of Movement*) contains an excellent critical review of the recent work on reflex inhibition (see especially 272 ff., 315 ff.).

[29] Cf. F. Fearing (A critique of the experimental studies of cortical inhibition with special reference to the knee-jerk, *J. Comp. Psychol.*, 7, 1927, 285-296) for a review of these factors.

[30] E. Jendrassik, Zur Untersuchungsmethode des Kniephänomens, *Neurol. Centralbl.*, 1885, 412.

[31] *Op. cit.*, 413.

The "Jendrassik Handgriff" became a standard clinical device. In 1886 Mitchell and Lewis,[32] making a study of the circumstances which increase or decrease the knee-jerk, noted a reënforcement of the jerk with simultaneous voluntary action, painful stimulation of the skin, and visual stimulation.

Lombard in 1888 studied the variability of the normal knee-jerk, with reference to the effects of fatigue, hunger, music and voluntary movement. He summarized his conclusions as follows:

> The causes of these variations of the knee-jerk are not only alterations in the muscles and nerves involved in the process, but, to a still greater degree, changes in the activity of the central nervous system, either as a whole or in part. Thus fatigue, hunger, enervating weather and sleep, conditions which decrease the activity of the whole central nervous system, decrease the average knee-jerk, while rest, nourishment, invigorating weather, and wakefulness, influences which increase the activity of the central nervous system, increase the average knee-jerk. . . . Thus voluntary movements and strong emotions, when synchronous with the blow, are found to increase the movement; and this is noticed even during sleep when the dreams are vivid. Similarly, sensory irritations, even when not strong enough to produce visible reflex actions, markedly reinforce the knee-jerk, but whether on account of their effect upon the brain, or upon the spinal cord, must be proved by future experiments.[33]

Investigation by Bowditch and Warren

The experiments of Bowditch and Warren[34] published in 1890, mark the most ambitious attack on the problem of facilitation and inhibition of the knee-jerk undertaken up to that time. The present importance of this investigation is evidenced by the fact that the conclusions are quoted as definitive by many modern workers. They appear in such physiological texts as those of Howell, Luciani, Ladd and Woodworth, and Schäfer. The wide-spread acceptance of the results of Bowditch and Warren as well as their theoretical importance warrants a reëxamination of the data and conclusions.

Mitchell and Lewis had noted that "the muscular action . . . . must precede the tap [on the knee], in order to reenforce it, by a period which is, as yet, undetermined." This suggested to Bowditch and Warren the importance of making a study of the effect of varying the interval between the reenforcing act and the stimulation of the patellar tendon. The reënforcing act consisted not only of a voluntary muscular contraction, but also of sensory stimulation—visual, auditory, and cutaneous. The muscular reënforcing act was a voluntary clenching of the hand in response to a bell stimulus; in the case

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[32] S. W. Mitchell and M. J. Lewis, Physiological studies of the knee-jerk and of the reactions of muscles under mechanical and other excitants, *Phil. Med. News*, Feb. 13, Feb. 20, 1886.

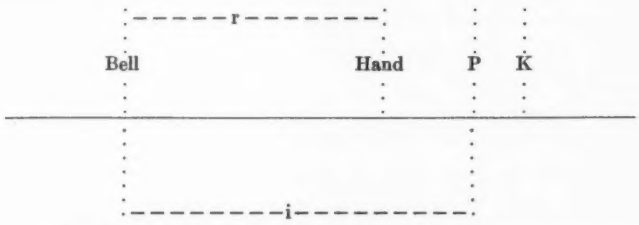
[33] Lombard, *op. cit.*, 68.

[34] Bowditch and Warren, *op. cit.*

of vision it was a flash of light caused by the sudden opening and closing of a stop-cock in a gas pipe supplying a burner in a lantern; the auditory stimulus was produced by dropping a large paper torpedo "near the head of the individual experimented upon," and the reënforcing cutaneous stimuli consisted of blasts of air directed to the conjunctiva, the mucous membrane of the nose and the skin of the neck.

The subject was placed on his side and the patellar hammer attached to the leg. The movement of the leg during the kick was recorded on a smoked surface by means of a system of levers attached to the foot. The apparatus was so arranged that the experimenter was able to control electrically the time between the reënforcing or auxiliary stimulus and the tap on the tendon. The reënforcing stimulus *always* preceded the tap.

The time sequence in the experiment may be represented schematically by the following figure.



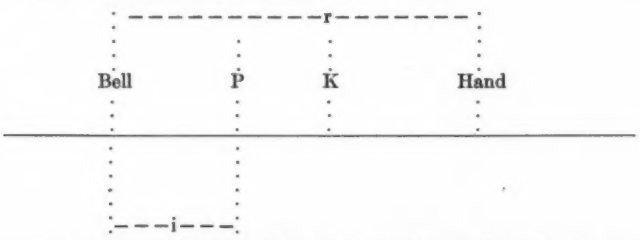
In the series in which a voluntary muscular contraction was the reënforcing stimulus, the subject was instructed to clench the right hand on a piece of wood in response to a tap on a bell. This is a voluntary reaction the time of which is designated as 'r.' The experimenter by means of the apparatus controls the time elapsing between the bell and the tap on the knee, 'P;' this time is designated as 'i.' The object of the experiment, then, is to determine the effect on the extent (amplitude) of the knee-jerk by varying the interval 'i.' Each series of experiments lasted about an hour during which varying intervals of 'i' were used interspersed by experiments in which there was no reënforcing act. These were called "normal" and the differences between the extent of the knee-jerks in the "normal" and reënforced series gave a measure of the reënforcing effect.

Tabulation of these differences according to the value of the interval 'i' showed "clearly that if the blow [on the knee] follows the signal [bell] at an interval not greater than 0.4 sec. the reënforcing act increases the extent of the knee-jerk. If the interval exceeds this amount a diminution of the knee-jerk re-

sults." If the interval 'i' was prolonged to 1.7 sec. the reënforcing act was without effect. From this the authors concluded:

> When by a brief act of volition the muscles of the forearm are innervated, the spinal cord is thrown into such a condition, that that portion of it which is concerned in the production of the knee-jerk is for a short time in a state of exalted activity, which is succeeded by a period of depression and then by a slow return to the normal state.[35]

The authors note that the value of 'r' is "always subject to considerable variation" and proceed to study the data when they are tabulated on the basis of 'i'–'r,' *i.e.* the interval of the time between the *beginning* of the reënforcing act and the blow on the knee. The authors note that the value of 'i'–'r' may be a minus quantity "in certain cases," *i.e.* the blow on the knee may have fallen *before* the subject has reacted to the bell stimulus. "In certain cases" seems a scarcely adequate characterization, since there are grouped in this category nearly half the measurements. The time sequence in these cases may be represented schematically as follows:



Inspection of the published data indicate that the tap on the knee, 'P,' may take place, in the cases where "the individual failed to reaction promptly to the signal," as much as 160 sigma before the clenching of the hand; since the time of the knee-jerk, 'P–K,' as measured by this technique cannot have been more than 70 sigma, it is clear that the knee-jerk was frequently completed *before the act which the experimenters had intended to to reënforce it.* In these cases the greatest amount of facilitation, as indicated by the extent of the knee-jerk, occurs.

Since the experiments of Bowditch and Warren are in a field which may be expected to yield much that is of major importance to both physiology and psychology, and since their conclusions are widely quoted in contemporary texts, a critical revaluation of their technique and results is in order. While such a critique is not the purpose of the present paper,[36] it may

[35] *Ibid.*, 38.
[36] Cf. Fearing, *op. cit.*

be pointed out in passing, however, that there is a possibility of the existence of factors operating to modify the amplitude of the knee-jerks other than the auxiliary muscular act itself.[37] The process of attention suggests itself in this connection.[38] This process, whether defined in terms of behavioristic psychology as a form of sensory adjustment which increases the effectiveness of stimulating conditions, or in terms of structural psychology as an increase in sensory clearness, might be expected to bring about nervous and muscular changes which would modify a coincident reflex, *e.g.* the knee-jerk. At any rate, the events in the Bowditch and Warren experiment, which occurred between the bell signal and the voluntary response thereto, would seem to be of primary importance in experiments of this type. Stated briefly, the question is: what was the attentional state of the subject just before and during the tap on the knee?

Sternberg is of the opinion that simultaneous voluntary muscular action exaggerates the knee-jerk, since it serves to direct the attention of the subject away from the knee; when the attention is directed towards the knee there is a tendency towards a voluntary contraction of the same muscle which is involved in the reflex. This, according to Sternberg, inhibits the reflex contraction.[39]

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[37]That is in the intact nervous system. In this connection the following quotation from a recent paper by Dodge (*Psychol. Rev.*, 33, 1926, 3) is pertinent: "Restricted in the method of direct experimentation to techniques that may be applied to intact and complex nervous systems, the experimental psychologist is peculiarly dependent on the results of physiological experiments with isolated tissues and simple systems both for his working hypotheses and his elementary facts. But he must take scientific responsibility for testing the extension of these hypotheses and facts to the complex conditions of human behavior, experience, and personality . . . . While it is often regarded as unfortunate that nerve fibers, nerve muscle preparations, and reflexes cannot be functionally isolated in the intact human on demand, it may not be forgotten that it is exactly in the mutual interrelationships of the various neural strata in human behavior and the integration which is called consciousness that there lies a group of problems of the utmost practical importance."

[38]E. B. Twitmyer, *A Study of the Knee-jerk*, 1902, suggests attention as an explanation of the observed variability in the knee-jerk. He says: "When the patella tendons are struck at exactly the same place with blows of constant force and at regular intervals, no two of the resulting knee jerks are of the same extent. This variation is usually referred to some accidental stimulus acting upon the subjects, e.g., a loud or distracting sound or to an idea to which the subject direct his attention, an emotional state of greater or less intensity any one of which conditions may produce a variation in the extent of the jerk, or in a general way to a change in the activity of the central nervous system. From the very nature of the case such sources of variation are not wholly avoidable. However secure the subject may be kept from accidental sensory stimuli, the stream of consciousness is never altogether within control of the experimenter and the organic processes of digestion, circulation, etc., are constantly producing some slight or more profound modification in the equilibrium of the nervous system" (p. 8 f.) Dodge in 1911 noted that attention "to some arbitrarily selected object in the environment diminished the jerk and rendered it relatively constant" (*op. cit.*, 25).

[39]Hoffmann, in his monograph on the "Eigenreflexe," *op. cit.*, objects to the conclusion of Sternberg that voluntary contraction of a muscle precludes its simultaneous reflex contraction, *i.e.* inhibits the reflex. Using the galvanometric technique and registering the action current in the muscle, Hoffmann shows that a simultaneous voluntary contraction of the extensors (*Agonisten*) facilitates the reflex, while a simultaneous contraction of the flexors (*Antagonisten*) inhibits the reflex.

The study by Twitmyer (1902) is one of the earliest attempts to study the variability of the "unaugmented or normal knee-jerk" in human subjects. Twitmyer found a wide range of individual differences in the amplitude of the kick (0–165 mm.). He noted that subsequent to the kick there are swings of the leg which are not to be ascribed to the pendular swings of the leg coming to rest. These after-kicks frequently exceeded the knee-jerk itself in amplitude and tended to increase in extent as the knee-jerks are repeated. Twitmyer suggested the following hypothesis which is significant as a contribution to the theories of knee-jerk variability.

> Let us assume that the repeated transmission of reflex excitation along a reflex arc, including the spinal segment, an afferent and efferent conduction path or from a given segment in the cord to a segment somewhat removed, develops a pathway offering increasingly less resistance. . . . A blow of constant force on the tendons will then result in a more and more intense stimulation of both the quadriceps and its antagonist. This increase will be relatively constant, consequently the extent of the initial kick is still checked by the simultaneous stimulation of the hamstring muscles. The actual extent of the kick, however, tends to increase somewhat with the continuation of experimentation. With the increased intensity of the reflex influence, the flexors are thrown into more violent contraction, which contraction now becomes an adequate stimulus to the sensory fibers terminating in the flexors and as a result there is a vigorous excitation to the cord and ultimately to the quadriceps. This excitation therefore contributes its quota to the second outward excursion of the leg and frequently is sufficiently vigorous to produce a kick larger than the initial kick. This view is fully corroborated by Sherrington's experiment in which he succeeded in greatly augmenting the extent of the knee-jerk by merely compressing the hamstring muscle when it is completely dissected away from its attachments, while the nerves were still intact.[40]

Twitmyer's study is especially interesting since it contains one of the first recorded observations on the phenomenon later included under the term "conditioning." He found that there appeared a decided kick of both legs following "a tap of the signal bell occurring without the usual blow of the hammer on the tendons."[41]

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[40] Twitmyer, *op. cit.*, 24.

[41] Because of their historical significance, Twitmyer's conclusions in this connection are worth quoting in full: "Can these movements, then, be looked upon as the true knee-jerk phenomenon in which the appropriate stimulus, *i.e.* a blow on the tendons has been replaced by a new and unusual stimulus? These movements are not the result of voluntary effort. Each subject gave unqualified testimony on this point. This testimony is further corroborated by the fact that the subjects were kept in absolute ignorance as to the nature of the experiments about to be performed and consequently did not know when to expect the blow and when not to expect it. . . . To the observer these movements resembled the knee jerk in every detail. The characteristic jerky or explosive appearance was very evident. . . . The occurrence of the phenomenon, therefore, depends upon the preliminary simultaneous occurrence of the sound of the bell with the kick produced in the usual way, *i.e.* a blow on the tendon. After a certain number of trials, the number varying for different subjects, the association of the sound of the bell and the kick becomes so fixed that the bell itself is capable of serving as a stimulus to the movement. Physiologically the repeated association of the functioning of the motor cells in the lumbar segment of the cord, upon which the kick immediately depends, with the excitation of centers in the nuclei of the medulla connected with the auditory conduction path, has resulted in developing a fixed relationship between them. . . . The results of the experiments herein reported, however, would seem to indicate that it is only after a habit of interaction between the two involved centers has been developed by repetition, *i.e.* when the connecting pathway of discharge has become well worn, that the sound of the bell alone is an adequate stimulus to the movement."

Recent Experiments on the Problem of Variability in the Knee-Jerk

The investigation by Bowditch and Warren was the first of the attempts to investigate the problem of *Bahnung* and *Hemmung* in the knee-jerk, in man. Among the contemporary investigators should be mentioned Tuttle, who, in a paper in 1924,[42] presented the results of a study of the effects of mental activity on the knee-jerk. Tuttle was especially concerned with the effects of attention:

> In view of the paucity of quantitative data on this subject it was decided to measure the knee-jerk under active and passive conditions of highly socialized forms of peripheral stimulation. We did not feel free to hazard a definition of what psychologists have generally agreed to include under the term "attention" or "mental activity" but preferred to assign to the subject a problem which we felt reasonably sure would release the type of response which most psychologists would agree required "attention" or was performed under "attentive attitudes."[43]

The general method of attack was to obtain records of tendon responses to uniform blows administered at a regular rate while the subject was in a passive condition as regards attention, alternated with periods of mental work. The mental work consisted of mental arithmetic, conversation, mental tests, and, in the case of the one subject, attention to a problem in electrical wiring. The conclusion of the investigator may be stated in his own words.

> The records from all subjects studied show that mental activity as defined in this paper increases muscle tonus as exhibited by the knee-jerk. . . . If mental activity of the problem type as used in this experiment involves the psychological factors of attention, then the conclusion seems justified that muscle tonus as measured by the knee-jerk is either (a) one of the factors in the attentive process, (b) a function of the attentive process, or (c) the process of attention itself.[44]

It would have seemed advisable to attack the problem at a simpler level; such mental activities as adding, solving complex problems—and even carrying on a conversation—involve a complex of factors which are difficult to isolate.

Tuttle and Burtt,[45] using Tuttle's technique for eliciting and recording the knee-jerk, studied the effect of emotional conditions on this reflex. Pleasant, unpleasant, and affectively indifferent words were visually presented to the subject who was required to react by giving an associated word. The object

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[42] W. W. Tuttle, The effect of attention or mental activity on the patellar tendon reflex, *J. Exper. Psychol.*, 7, 1924, 401-419.

[43] *Ibid.*, 401.

[44] *Ibid.*, 418.

[45] H. E. Burtt and W. W. Tuttle, The patellar tendon reflex and affective tone, this Journal, 36, 1925, 553-561.

of the study was to determine the effect on the amplitude of the knee-jerk of the various types of words presented. The conclusions of the investigators are stated as follows:

> There is a depression of the reflex on the average for unpleasant stimulus words amounting to 16%. There are slight and less consistent indications of a similar depression for pleasant stimulus words. All the unpleasant categories used were effective in some instances in producing depression. . . . The correlations between the extent of a reflex and the association reaction-time for the stimulus word presented in connection with it are small, but all those that are of possible statistical significance are negative. This result suggests that the depression of the reflex and the slowing of the association time involve the same aspect of the stimuli, presumably the affective tone.[46]

In these experiments the subject reacted in every case *before* the knee was stimulated. The assumption was, presumably, that the subject's attention during the interval between the vocal response and the tap on the knee, was occupied with the word or with his response. In any event this period was critical so far as the knee-jerk is concerned. In spite of these objections these experiments present significant results. The conclusion cannot be escaped that mental processes or the neuro-muscular accompaniment of such processes included under the term attention significantly modify the knee-jerk.

A consideration of these problems leads, of course, to the larger problem of inhibition as it involves all neuro-muscular performances.[47] The field of inhibitional theory has become highly controversial, and as yet no one theory may be said to have behind it the weight of majority opinion. The 'drainage' hypothesis of McDougall has received attention from the psychologists. This hypothesis was presented by McDougall in 1903,[48] and assumes that "inhibition of the excitation of one neural system is always the result of the excitement of some other system, that inhibition appears always as the negative or complementary result of a process of increased excitation in some other part."

Dodge used the knee-jerk in testing the 'drainage' hypothesis of McDougall. He stated the question to which the experiment should give a specific answer as follows:

> Is it or is it not a fact that decrement of a reflex regularly occurs as a correlate of increased spread of neural excitation? Is it or is it not a fact that spread of excitation so as to involve the cortex or the initiation of a cortical reaction actually inhibits a related reflex? . . . If the in-

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[46] *Ibid.*, 561.

[47] The recent papers of E. D. Adrian (*Brain*, 47, 1924, 399-419) and C. S. Sherrington (*Proc. Roy. Soc.*, 97B, 1924-25, 528) contain summaries of the facts and theories in connection with inhibition.

[48] W. McDougall, The nature of the inhibitory processes within the nervous system, *Brain*, 26, 1903, 153.

hibition of a reflex by drainage actually follows the spread of excitation in the cortex, then it ought to be possible to inhibit a reflex by voluntary reaction to the same stimulus that normally excites it.[49]

To test the last statement, Dodge made use of the patellar reflex and the reflex eyelid reaction. He caused his subject in the case of the knee-jerk to respond by a voluntary finger movement to the same stimulus which released the knee-jerk. His assumption is that the knee-jerks which were accompanied by the simultaneous voluntary reaction would be consistently diminished in extent if the 'drainage' theory held. The results, for the few cases presented, were uniformly negative for the 'drainage' theory. Additional experiments are undoubtedly necessary in order to test the 'drainage' hypothesis. Wiersma[50] has recently pointed out that simultaneously functioning systems, *e.g.* the postural reflexes, may be expected to modify the knee-jerk; that is, those conditions which inhibit the postural reflexes facilitate the knee-jerk.

Golla and Hetwer[51] are among the contemporary investigators who have noted the variability of the magnitude of the knee-jerk. They believe that the greatest factor contributing to this variability in the intact human nervous system is cortical activity. This factor, they believe, may be rendered negligible by taking "suitable precautions." If the process of attention may be included under "cortical activities," the experimental control of this process would seem to be essential.[52]

In addition to inhibition, the knee-jerk has been used by recent investigators in testing for the effects of alcohol, fatigue and other modifying agents. The knee-jerk was found by Tuttle[53] to be depressed or absent during sleep. This confirms the results of Rosenbach, Bowditch and Warren, and Lombard in the previous century, and Lee and Kleitman[54] in 1923.

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[49]R. Dodge, Theories of inhibition, *Psychol. Rev.*, 33, 1926, 118f.

[50]E. D. Wiersma, Die psychologische Auffassung einiger Reflexe, *Zsch. f. d. ges. Neur. u. Psychiat.*, 72, 1921, 254-266.

[51]F. Golla and J. Hetwer, The influence of various conditions on the time relations of tendon reflexes in the human subject, *Proc. Roy. Soc.*, 94B, 1922, 92.

[52]Golla and Hetwer in another paper (*Brain*, 47, 1924, 57-69) have presented the results of a study of the time relations involved in the coördination of muscles in voluntary and reflex responses. The Einthoven galvanometer was used. The following quotation from this paper is pertinent: "The time relations of the coordination of various muscles in certain voluntary movements is the same as when a similar movement is elicited as a spinal reflex response. Electromyograms of the patellar and the Achilles reflexes were obtained, recording the contraction of the antagonistic muscles concerned. It was found that in the patellar reflex the contraction of the flexor muscles of the knee follows that of the quadriceps extensor at an interval of 7 to 10 sigma. If a voluntary kick in imitation of a knee-jerk be made, it will be seen that the flexor contraction follows the extensor in 10 sigma, that is, in about the same time" (p. 66).

[53]W. W. Tuttle, The effect of sleep upon the patellar tendon reflex, *Amer. J. Physiol.*, 68, 1924, 345.

[54]M. A. M. Lee and N. Kleitman, Studies in the physiology of sleep, *Amer. J. Physiol.*, 67, 1923, 141-152.

Alcohol also acts as a depressant according to Dodge and Benedict,[55] Miles,[56] and Tuttle,[57] all of whom have studied the effects of the ingestion of alcohol on the knee-jerk. Tuttle found that alcohol may have either an excitatory or depressant effect; of the 12 cases studied, 9 showed augmentation and 2 depression of the knee-jerk, and one case was unaffected.

Although not concerned with the problems just referred to, the recent papers of Sherrington and Liddell[58] mark an important advance in knee-jerk research. These observers demonstrated that a gradual stretch applied to a muscle (especially the anti-gravity muscles) evokes a contraction. The knee-jerk and the patellar clonus are regarded as "fractional examples of the more comprehensive stretch reflex." In other words, the knee-jerk is produced by a very slight stretch of extremely brief duration. A stretch of less than 1/20 mm., lasting less than 1/20 sec., is sufficient to produce the jerk. The term 'myotatic' is applied to these reflexes which include postural, tendon and tonic reflexes. The work of Sherrington and Liddell bring under a single explanatory principle, phenomena which have appeared to be divergent hitherto.

Conclusions

What is the result of more than fifty years of experimental work on the knee-jerk? Only an attenuated sample of the huge literature on the subject of the patellar reflex has been presented here; one large section of that literature, which refers to the clinical aspects, has been neglected entirely. Because of the relative ease with which the knee-jerk may be controlled experimentally, it has perhaps received more laboratory attention than any other tendon-reflex. The researches typified by the work of Lombard, Bowditch and Warren, Dodge and others have attacked fundamental problems in the field of neuro-muscular action. The knee-jerk would seem to offer an excellent means of studying some of the 'urgent' problems of physiology and psychology to which Sherrington has referred.[59]

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[55] *Op. cit.*

[56] *Op. cit.*

[57] W. W. Tuttle, The effect of alcohol on the patellar tendon reflex, *J. Pharmacol. and Exper. Therapeut.*, 23, 1924, 163-172.

[58] C. S. Sherrington and E. G. T. Liddell, Reflexes in response to stretch (myotatic reflexes), *Proc. Roy. Soc.*, 96B, 1924, 212-242; Further observations on myotatic reflexes, *ibid.*, 97B, 1925, 267-283, 488-518.

[59] "In other words, the reactions of reflex-arcs are controllable by mechanisms to whose activity consciousness is adjunct. By these higher centres, this or that reflex can be checked, or released, or modified in its reaction with such variety and seeming independence of external stimuli that the existence of a spontaneous internal process expressed as 'will' is the naïve inference drawn. Its spring of action is not now our question; its seat in the nervous system seems to correspond with that of processes of perceptual level. It is urgently necessary for physiology to know *how* this control—volitional control—is *operative* upon reflexes, that is, how it intrudes and makes its influence felt upon the running of the reflex machinery. . . . No exposition of the integrative action of the nervous system is complete, even in outline, if this control is left without consideration." *Integrative Action of the Nervous System*, 1906.

It would seem that, among others, problems involving the relation of a concomitant mental process, or, stated in neural terms, the relation of the function of concomitant cortical systems to the knee-jerk in the intact human nervous system, might approach solution. On the other hand, there are serious technical difficulties, especially in connection with the standardization of the stimulus for the reflex, and the adequate registration of the response. Dodge said in 1910 that as heavy and clumsy a lever as the leg "weighing fifteen pounds or more" would be "laughed out of court in any other physiological experiment." Tuttle, however, has reported that when the excursion of the leg is recorded in knee-jerk experiments, the chief difficulty has been "in the sensitiveness of the method."

It has been pointed out that whatever may be one's definition of psychology, there can be no doubt that its province lies in part, at least, in the examination of the behavior of the animal with an intact nervous system, and that the psychologist must take the scientific responsibility for extending the results from the functionally simple physiological systems to the complex conditions of "human behavior, experience and personality."[60]

Provided the problems of experimental technique are solved, the knee-jerk would seem to one of the responses available for study in connection with the functional relationship between 'higher' and 'lower' physiological systems.

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[60]Dodge, *op. cit.*, 3 ff.

RECALL *VS.* REPETITION IN THE LEARNING OF ROTE AND MEANINGFUL MATERIAL

By Wm. Clark Trow, University of Michigan

This experiment, the object of which was to compare the value of recall and repetition in the learning of rote and meaningful material, was performed with the aid of 10 graduate students in educational psychology who acted as subjects. Three kinds of material were presented to the *Ss* under uniform conditions: (1) series of digits; (2) lists of monosyllabic concrete and abstract nouns; and (3) a descriptive paragraph.

The digits were arranged in 19 series, 4 digits in the first series, 5 in the next, etc. They were presented orally by *E* at the rate of one per sec.; the *Ss* recorded each series as soon as it was completely presented. The lists of nouns, taken from Whipple's Manual,[1] were given out similarly one list at a time, first 3 words, then 4, etc., up to 9, the last list being composed for the purposes of this experiment. The paragraph used was *The Old Homestead*.[2] This was read at a uniform rate clear through, after which the *Ss* wrote down as much as they could remember.

Following the initial presentation just described, all *Ss* had 7 practice periods of different sorts. The 9th period took place one day after the last practice, the 10th one week after, and the 11th 15 weeks after. Thus it was possible to discover the permanence value of the different methods used for the different materials.

After the initial presentation, which was the same for all, the practice period, held daily except Sunday for all but Group V, differed for each group of *Ss* as follows:

Group I. Control Group. At each practice period, the *Ss* wrote as much as they could remember without having the material presented to them again.

Group II. Recall Group. At each practice period, the *Ss* first wrote as much as they could recall from the previous presentations, and then listened without writing while the materials were presented to other *Ss*. The procedure here followed differs from that of Group I in that progressive improvement was possible since the materials were presented each day. But this presentation did not occur until *after* the *Ss* had recalled as much as they could from the previous days. They were thus forced to employ the method of recall in learning.

Group III. Re-presentation Group. At each practice period the *Ss* listened and then wrote as in the first presentation. Here, it is observed, there was no necessity for recall from the preceding presentations. Each set of digits, each list of words, etc., was written down at once as soon as it was given out.

Group IV. Recall–Re-presentation Group. A combination of the methods of II and III. At each practice period the *Ss* wrote as much as they could recall from the previous presentations like Group II, then listened and wrote like Group III. These *Ss* to aid their recall each time had the benefit not only of having *heard* the materials like Group II, but also of having gone through the exercise of writing them, like Group III.

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[1] G. M. Whipple, *Manual of Mental and Physical Tests*, 2, 1915, 164 f.

[2] *Op. cit.*, 210.

Group V. Massed Group. Like Group III, the *S*s listened and then wrote, but all the practice periods were run through at one sitting, each period following immediately upon the conclusion of the preceding. (Unfortunately it was not possible to get the figures for this group for the one-week period.)

The method of scoring was as follows: every digit counted 4 in its correct place in the series; if displaced one place, 3; two places, 2; three places, 1; more than three places, 0. The same method was employed for the words except that every word reproduced correctly however far out of position was given a score of at least one. The paragraph was scored for 'ideas' according to the key in Whipple's *Manual*, and for every noun reproduced scoring one, the context necessarily making the setting practically correct. The *S*s were not asked to underline the words they were certain of, as in Whipple's directions, because this procedure would furnish data irrelevant to the problem.

TABLE I

		Digits			
G	*S*	*Immediate*	*1-day*	*1-wk*	*15-wks*
	Le	188	0	0	0
I Control	Co	192	6	6	0
	Bl	155	27	18	0
II Recall	He	178	57	42	3
	Sa	165	0	0	0
III Re-presentation	Br	172	22	8	18
	Fe	158	18	9	0
IV Recall–Re-pres.	Ed	171	54	75	57
	Ha	157	47	—	0
V Massed	Be	168	81	—	6

Table I shows the score for digits remembered by every *S*. The first column of figures gives the total score on the initial presentation when the digits were written down immediately after each series was given out. The average of these scores is 170.4 and the average deviation is 9.8, showing how close together the abilities of the *S*s are in this respect. The other three columns represent the total number of digits remembered after the practice for a period of 1 day, 1 week, and 15 weeks. The plan of the other tables is similar to this, including the arrangement of groups in the order of excellence of retention after 15 weeks, though the zero recollection of one *S* in each group for this difficult rote material makes comparison in this case fruitless.

Since Groups II and IV had a quarter more time than the others (2 min. instead of 1.5), in order to make them comparable with the others their scores in the table are corrected, being in each case ¾ of the raw score.

The superiority of Group II (Recall) and Group IV (Recall–Re-presentation) for the 1-day and the 1-week period seems clear, as does the superiority of the massed method over all others for one day. The performance of the *S*s in Group III, who heard the digits repeated daily, was nearly as meagre as that of those in Group I who heard them but once.

Fig. 1 shows the average performance for each group for the trials given in Table I.

Table II shows the results in the same way for the word lists. The average score of the first trials is here 245.5, and the average deviation

TABLE II
Words

Group	S	Immediate	1-day	1-wk	15-wks
I Control	Le	232	2	2	1
	Co	296	0	0	0
II Recall	He	245	42	34	30
	Bl	256	150	162	70
III Re-presentation	Sa	246	49	58	20
	Br	262	67	24	7
IV Recall–Re-pres.	Fe	233	80	104	50
	Ed	225	64	72	44
V Massed	Ha	200	109	—	4
	Be	260	225	—	30

TABLE III
Percentage of Total Words Remembered Which Were Abstract

Group	S	Immediate	1-day	1-wk	15-wks
I Control	Le	49	50	50	0
	Co	47	—	—	—
II Recall	He	47	33	29	20
	Bl	47	29	35	32
III Re-presentation	Sa	45	28	38	15
	Br	49	21	50	71
IV Recall–Re-pres.	Fe	48	41	51	13
	Ed	51	38	39	30
V Massed	Ha	51	38	—	25
	Be	49	46	—	0
Total Score Ab. Wds.	All	1184	343	256	82
Total Score Words	All	2455	958	643	354
% Total	All	48	36	40	32

TABLE IV
Paragraph-Nouns

Group	S	Immediate	1-day	1-wk	15-wks
I Control	Le	19	21	22	22
	Co	20	15	18	18
II Recall	He	7	39	41	26
	Bl	11	44	46	40
III Re-presentation	Sa	23	43	45	32
	Br	14	43	36	33
IV Recall–Re-pres.	Fe	7	25 (41)	26 (43)	20 (33)
	Ed	13	25 (42)	28 (46)	23 (39)
V Massed	Be	16	43	—	24
	Ha	12	40	—	23

is again small, being 18.5. The superiority of Groups II and IV is shown even when the raw score is reduced ¼ to correct for the added time employed by these *Ss*.

The striking advantage of the massed method is to be noted for the one-day period, as before, and its uselessness for the 15-week period. Group III (Re-presentation) again appears as inferior.

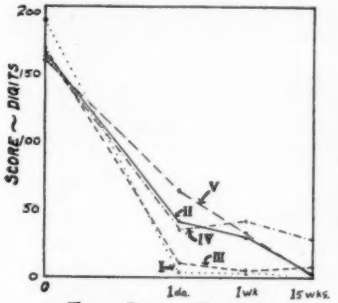

FIG. 1. Digits-Averaged

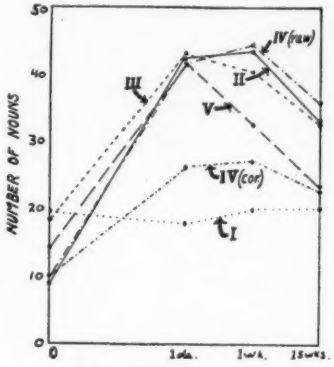

FIG. 3. Paragraph-Words

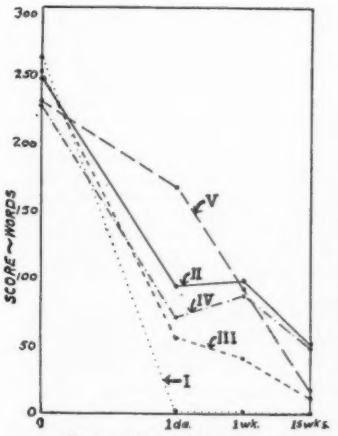

FIG. 2. Words-Averaged

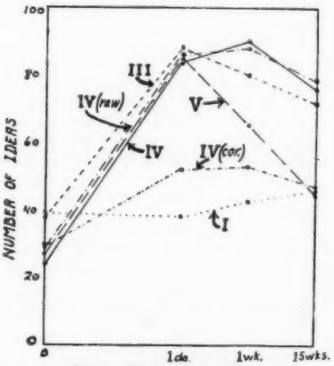

FIG. 4. Paragraph-Ideas

Fig. 2 shows the average performance for each group for the trials given in Table II.

Of the total number of words remembered by each *S*, Table III shows the percentage that were abstract. Fifty percent would mean that an *S* scored as high for abstract words as he did for concrete. There seems to be no marked relationship between the percentages and the methods of learning. It is not easy to see the cause for the increase in the percentage of abstract words remembered as compared with concrete over the 1-week

interval. Relatively fewer abstract words are remembered after the 15-week interval.

Table IV shows the number of nouns remembered when they were presented in paragraph form as meaningful material rather than as lists. The average of the first trials is 14.2 and the average deviation is 4.2. The amazing thing here is the negligible superiority of all groups after 15 weeks to the *Ss* in the Control Group.

The paragraph was so nearly learned that repetition at the end of the training period, as shown in the 1-day column, was practically perfect, the total number of nouns being 47. There is here little differentiation between groups.

Though Group IV had .4 more time than the others, the performance after the first three or four trials of the practice period was so nearly perfect that the presentation of .6 of the score as a correction in this case is clearly not just. The raw scores are therefore given in parentheses.

TABLE V
Paragraph Ideas

Group	*S*	*Immediate*	*1-day*	*1-wk*	*15-wks*
I Control	Le	40	36	44	48
	Co	39	40	43	44
II Recall	He	16	77	87	66
	Bl	32	91	93	86
III Re-presentation	Sa	33	89	86	69
	Br	43	88	74	75
IV Recall–Re-pres.	Fe	19	50 (84)	50 (84)	44 (74)
	Ed	40	54 (90)	55 (92)	50 (84)
V Massed	Be	36	86	—	47
	Ha	19	83	—	43

Table V in similar fashion presents the number of 'ideas' remembered by the different *Ss* over the different intervals. The average initial performance is 31.7, the average deviation 8.2. The highest possible score is 95. Here even more clearly than elsewhere is shown the comparative success of the massed method for recall one day later, and its utter uselessness fifteen weeks later. There is practically no difference in the results when the nouns are counted as in Table IV and when the 'ideas' are counted as in Table V.

The same fact can be read in Figs. 3 and 4. The groups retain practically the same relative position, too, that they do in the recall of the word lists, the most marked difference being, in the case of the lists, the superiority of the massed method after one day and the consistent inferiority of the re-presentation method.

CONCLUSIONS

(1) The Method of Recall (Groups II and IV) is superior to other methods.

(2) The Method of Re-presentation without Recall (Group III) is consistently ineffective.

(3) The Massed Method is as good as any for meaningful material and better than all other methods for rote material for one day's retention, but quite useless for retention over much longer periods.

(4) Abstract nouns are about equal to concrete in immediate recall, but after one day are forgotten to about the same extent that the concrete nouns are after fifteen weeks.

THE EFFECT OF PRACTICE ON DIFFERENT DEXTRALITY TYPES

By HAZEL BOWMAN, University of Wyoming

The objects of the present experiment were (1) to investigate the effect of practice upon the records made by each hand in a pegging test by individuals of different handedness formulae; and (2) to compare the performances in order to determine whether there are significant differences which may be correlated with the differences in handedness.

Subjects. Five *S*s, all women, were used. They had been used by Downey in her study of dextrality types,[1] and had been classified by her according to the handedness and eyedness formulae as follows.[2]

S	Formula
R	RRR–Right-eyed
D	RRL_2–Left-eyed
C	RRL_1–Either-eyed
B	RLL–Either-eyed
M	LLR–Left-eyed

The formula RRR represents an individual who is right-handed in both unimanual and bimanual activities, writes right-handed, uses a bat right-handed, and puts the right hand down when using a broom.

An RRL individual writes and bats right-handed but puts the left hand down on the broom. There are two subforms of the RRL type; the RRL_1 individual reports that in using the broom the work is done primarily by the right hand; the RRL_2 individual does the work primarily with the left hand.

Those who are of the formula RLL write with the right hand, bat left-handed, and put the left hand down on the broom.

A left-handed individual, LLR, uses the left hand in all unimanual activities, bats left-handed, and puts the right hand down on the broom. M has been forced by social pressure to use the right hand in writing; she writes rapidly and in excellent form.

The *S* was seated before a 'primary pegging-board,' 10 in. sq., containing 100 holes in 10 parallel rows and given a supply of wooden pegs. One hand was used to hold the pegs, the other to place them in the holes. When pegging with the right hand the *S* worked from left to right; when using the left, from right to left. The hands were used in the following order: R L L R R L. One minute was allowed for every trial. Every *S* was put through 35 sets of trials (a set being 3 trials with each hand in the above order), they thus had 105 trials with each hand). R and C were given three sets of trials a day on alternate days; a 5-min. rest being given between every set. B, D, and M were given one set a day on succeeding days. The average number of pegs put in by each hand, and the ratio between the two hands, R/L, was calculated for each set of trials.

*Accepted for publication February 25, 1927.

[1] J. E. Downey, Types of dextrality and their implications, this JOURNAL, 38, 1927, 317-367.

[2] Subject M was tested by Carol Rigby.

The results were summarized for two of the *Ss* in Fig. 1, and for all the *Ss* in Table I. The practice curves show a fairly steady increase in the number of pegs put in by each hand for all *Ss*. The table shows that there was, however, a decrease in the ratio R/L for all right-handed *Ss*, though this is partially due to an increase in the absolute values. The right hand,

TABLE I

Showing for every *S* the average performance with each hand and the ratio between them

S	Formula	First Set			All Trials		
		R	L	Ratio	R	L	Ratio
R	RRR–R E	53.33	32.66	1.632	76.41	58.50	1.319
D	RRL_2–L E	48.33	34.33	1.407	65.24	60.10	1.099
C	RRL_1–E E	37.33	33.33	1.120	60.48	55.17	1.102
B	RLL–E E	70.00	55.66	1.257	80.70	66.76	1.175
M	LLR–L E	38.66	49.33	.783	58.35	71.42	.814

as one might anticipate, appears to reach its approximate physiological limit more quickly than the left. For the left-handed *S* the ratio increases, showing the same shift in the performance of the preferred and non-preferred hand.

A comparative study of the records is interesting. Table II gives the relative order of the *Ss* in the performance of each hand before and after the practice series.

It will be seen from Tables I and II that, of the right-handed *Ss*, B (formula RLL) made the fastest record with both hands. M, the left-handed *S* ((formula LLR), surpassed B after practice in the performance of the left hand. R (formula RRR) was consistently high in the perform-

TABLE II

Showing the relative order of the *Ss* in the performance with each hand before and after practice

Right Hand		Left Hand	
Before Practice	After Practice	Before Practice	After Practice
B	B	B	M
R	R	M	B
D	D	D	D
M	C	C	R
C	M	R	C

ance with the right hand and low with the left. D and C (RRL_2 and RRL_1 respectively) did not have the speed of B with either hand, but they both surpassed R in the performance of the left hand before practice, and D even after practice. M, as was to be expected, was consistently high in the speed of the left hand and low in that of the right.

A study of the practice curves shows another interesting fact. The ratio of the right to the left hand is not the same for all the *Ss*. B and R were consistently faster with the right hand, whereas D and C with the left hand equalled or surpassed the record with the right one or more times in the series. The practice curves of R and of D, shown in Fig. 1, are very unlike.

The records show that R (formula RRR) is more unilateral in her performance than are the RRL individuals. She never makes a ratio less than 1.205. The mean ratio is high, 1.319, and at no time is there any evidence of a left-handed advantage. D and C (the RRL *Ss*), on the other

hand, give a low mean ratio (1.099 and 1.102 respectively) and throughout the practice series the performance of the left hand follows closely that of the right—even surpassing the right hand record, as has been said, at one or more points. The introspections of these *S*s, to be reported later, also give evidence of frequent left hand advantage.

The records of B are not easy to interpret. Perhaps the fact that the greatest speed is made by this *S* is significant. The pegging test is not

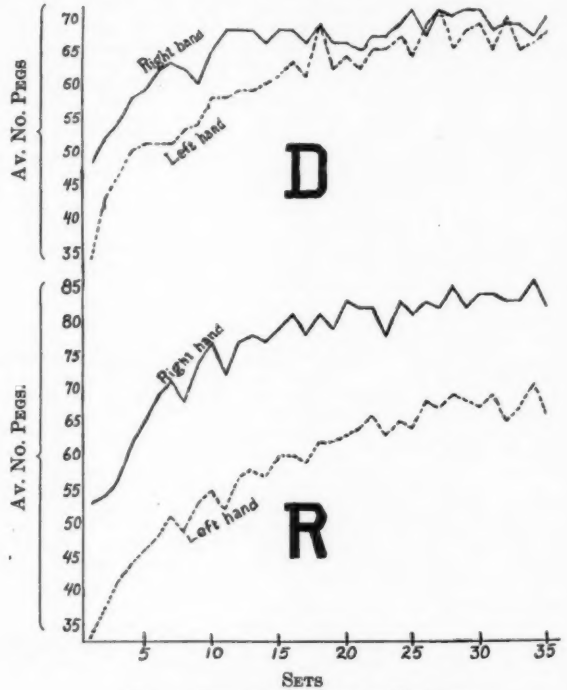

FIG. 1. PRACTICE CURVES IN PEGGING
D's formula is RRL_3–Left-eyed
R's formula is RRR–Right-eyed

'strictly a unimanual performance and the skill of this subject in 'feeding the pegs by the hand not pegging undoubtedly operated to increase the number of pegs placed.

Certain additional data were obtained from the *S*s which may be significant. B is classified as either-eyed. Apparently in her case, eye-dominance fluctuated from day to day. The question arose as to whether dominance of right or left eye influenced the relative performance of the two hands. A report on eye-dominance was obtained from B and the performances for the right-eyed days and left-eyed days compared. The average performance during the right-eyed days for the right hand was

79.52, for the left hand, 67.22,—a ratio of 1.183. The average during the left-eyed days for the right hand was 79.89, for the left hand, 68.35—a ratio of 1.169. Though the ratio for the left-eyed days is smaller than for the right-eyed days, the difference is slight and the results are of doubtful value. Moreover, it is quite possible that eye-shifts occurred with the shifts of the pegging hand.

D and C showed many left-handed tendencies. They reported in this connection, definite but fluctuating 'feelings' as to handedness, depending upon a feeling of greater kinaesthetic readiness in one or the other of the two hands. They spoke of "right-handed days" and "left-handed days." D reported a difference on different days "in which hand goes out;" that is, which hand is used in the performance of tasks, such as opening a door, etc. To determine whether these 'feelings' represent a variation in the relative motor skill of the two hands, a report was obtained on this point from D. Out of the 35 days, 13 were reported as 'right-handed' days, 7 as 'left-handed' days. For 15 days there was no definite feeling. The average number pegged on the 'right-handed' days with the right hand was 69.92, with the left hand, 60.25—a ratio of 1.111. The average number pegged on the 'left-handed' days with the right hand was 65.04, with the left hand, 60.90—a ratio of 1.068. Here again the difference between the ratios is too small to be significant.

Before drawing any conclusions from the entire investigation it may be well to point out criticisms of the procedure. (1) The number of cases is so small that shifts in relative position mean little. (2) It appears from observation that the pegging test is not a unimanual operation. The skill of the hand not pegging operates to increase or decrease to a great extent the number of pegs put in. It is interesting to note that in most of the right-handed *Ss* the left hand was much more skillful in 'feeding,' and often during the practice series it was evident that the great increase in the speed of the left hand actually represented an improved technique on the part of the right in furnishing the left hand with pegs. C was very inefficient in feeding pegs, an inefficiency which cut down her speed to a great extent. R and B were exceptionally skillful in this regard, though the left hand of R performed this operation much more easily than the right. (3) A comparative study of the *Ss* is complicated to some extent by the difference in effort put forth. Although the *Ss* were told to work as fast as possible it was evident that the instruction was followed more closely by some than by others, dependent upon the motivation. C cared not at all whether her record was fast or slow; B and D put forth the greatest possible effort. A desire to excel with one or the other hand also operated to influence the results.

Conclusions

A study of the records made in the pegging test by *Ss* of different handedness formulae over a practice series of 35 sets of trials suggests the following conclusions:

(1) All the *Ss* showed a practice effect—there is an increase in the number of pegs placed in the pegging-board in the 1-min. periods. There was a decrease in the R/L ratio for all right-handed *Ss*, and an increase in the ratio for the left-handed *S*.

(2) R, whose handedness formula is RRR, appears to be more unilateral in performance than D and C, RRL *Ss*.

(3) The record of B, whose handedness formula is RLL, is difficult to interpret. Her speed in pegging is the greatest of all our *Ss*. Since pegging, as it was done by our *Ss*, may be a bimanual performance, her speed may be significant of her handedness formula.

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By KARL M. DALLENBACH, Cornell University

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APPARATUS

THE YALE PSYCHO-CLINIC PHOTOGRAPHIC OBSERVATORY

By H. M. Halverson, Yale University

Dr. Gesell has for some time recognized the value of the camera as a tool for the scientific study of children, and to this end has had in mind the utilization of a mechanical device[1] which permits photographing subjects from the most advantageous angle.[2] The method of advancing upon an *S* and photographing from the conventional camera level does not always yield an optimal return, when one considers that one desires a truly representative record for further intensive study. Ideally, one should be in position to photograph instantly from any angle the *S*'s behavior. The two simultaneous photographs of the *S*, shown in Fig. 1, illustrate the point in question. *B* shows the subject from the eye level and *A* shows the overhead view. Our experience with infants is that overhead views are likely to prove more valuable than are horizontal views. An attempt to meet the requirements above mentioned led to the construction of the apparatus herein described.[3]

The photographic observatory is essentially an hemispherical iron-ribbed structure, inside diameter 12 ft. 6 in., mounted so that it rotates on a substantial cylindrical iron base, 32 in. high, inside diameter 12 ft. For brevity we shall refer to this entire structure as the dome. Sixteen T-iron ribs of the superstructure, with the stem of the T exterior, radiate from the top, meridian-like, 20° apart, and are bolted at the other end to a heavy horizontal 2-in. angle-iron, which forms the circumferential support of this upper portion. An iron band, 1 in. wide, encircles the structure midway of the top and base and acts as a brace for the T-ribs. The cylindrical base upon which the superstructure rests consists of a heavy 2-in. angle iron bent in the form of a circle, which sets on the floor, upon which at 9-in. intervals are erected vertical ½-in. square iron posts which support the heavy U-iron of the same form and diameter as the floor iron. The entire piece is riveted firmly together into a substantial base.

The superstructure overlaps the cylindrical base exteriorly by 2 in., and rests upon 7 strong 2-in. fibre pulleys which are bolted to the base near its top at equally spaced distances. The center of the dome is therefore exactly 30 in. from the floor, giving the entire structure a total height of 9 ft. 6 in. Seven other pulleys, fastened horizontally to the base above the first pulleys, protrude ⅜ in. beyond the outside of the base and serve as guides to prevent the upper portion of the dome, which can be rotated through an arc of 360°, from scraping against the base. The dome, except for the camera tracks and the windows which contain the lamps (described below), is covered by a 16-mesh iron screen, wired securely to the iron frame-work.

Two of the T-ribs of the dome, 90° apart, give place to camera tracks, the left[4] one of which extends to the top of the dome through an arc of 90°, the other not quite to the top. Each track consists of two U-irons bent to fit the form of the dome with the hollows of the U facing each other the length of the track. The camera carriage, an oak board 6 x 10 in. mounted

[1] Arnold Gesell, *Mental Growth of the Pre-School Child*, 1926, 160.

[2] A complete description of the procedure and the apparatus used in the clinical examination will be found in Gesell's forthcoming book.

[3] The writer acknowledges with thanks the helpful suggestions by Professor Raymond Dodge.

[4] All directions are exterior.

on six wheels, four on the bottom and two on the top on springs, travels within this grooved track. The top wheels press elastically against the rear flanges of the track and thus automatically force the bottom wheels against the front flanges so as to hold the carriage at right angles to the center of the dome. The camera is screwed firmly to an oak shelf 7x9 in., hinged exteriorly to the upper end of the carriage, and is adjustable to any vertical angle with respect to the inside of the dome by means of a slot-and-screw device. The carriage is operated by a cord and pulley, while a special spring-brake locks it firmly to the track at any point.

A battery of 8 Cooper-Hewitt A. C. lamps, supplemented by twenty-four 50-watt incandescent lamps, mounted in three asbestos-lined boxes which fit the curvature of the dome and rest on the T-ribs, supplies the light. Two of the boxes, each containing three Cooper-Hewitt and nine incandescent lamps, lie adjacent midway of the two camera tracks at an angle of 40° with the horizontal. The third box containing the remaining lamps lies 25° to the left of the left camera track at an angle of 50° with the horizontal. Photographically, the two large boxes serve as the light source, while the smaller box is the reflecting screen. Special flexible wire cables, supplying the energy for the lights, pass through a ring hanging from a ball-and-socket joint in the ceiling immediately above the dome and thence down to the boxes containing the lamps. This arrangement permits the conductors to turn about freely as the dome rotates.

White tissue-paper screens, following the form of the dome, shield the *S* from the direct glare of the lights and remove distraction from this source. The incandescent lights, although they have little actinic value, serve to add the red rays absent in the Cooper-Hewitt lights and thus restore to objects their normal appearance. Two light grey window-shades on rollers which are suspended in the dome, one at right angles to each camera, serve as background screens.

The two still cameras are operated electrically by a six-volt storage battery which energizes an electro-magnet on the camera shelf above which projects an iron extension of the camera trigger which acts as an armature. The photographer may operate one, or the other, or both cameras simultaneously, regardless of their positions, by a simple manipulation of the control switches and contact key. The error in simultaneity of action of the cameras, as measured by a 100 d.v. electric fork, is less than 0.01 sec.

The entrance, 6 ft. 9 in. high and of the width of two segments of the dome, is opposite the right camera track. To enter one pulls outward two swinging doors and the hinged angle iron support of the superstructure and a gate of the cylindrical base.

The nature of the dome permits standardization and experimental control to a considerable extent. The fine mesh screen covering the dome, with its coat of flat white paint, serves a three-fold purpose in that it provides excellent ventilation, salvages most of the light, and makes possible observation from the outside without detection by the *S* under examination. The camera tracks and the base on which the superstructure rotates are marked off in degrees so that one may quickly place the camera at any desired angle. The right angular distance between the two cameras checks the third dimension. A cinema camera mounted on a special carriage which may be fitted to either track at any point or projected through the door or apertures in the wire screen gives a continuous record of the *S*'s behavior. The arrangement of the still and motion cameras permits an elastic program in photography. The use of artificial lamps assures dependable light for photographing at high speed. The constant relation between the light source and cameras yields comparable photographic records. The practically noiseless operation of the lamps and dome is an asset to any form of experimentation.

The dome has been in operation since March, 1927, and its usefulness amply demonstrated. To date our products include many hundreds of still photographs and several thousand feet of motion film of the 16 mm. variety. Besides the relatively large number of children whom we photograph for the purpose of securing norms of behavior, we make special intensive studies of a few *Ss*, each of which is examined at intervals of four weeks. A single examination yields from 30 to 60 still photographs and from 100 to 200 feet of motion film.

A DEVICE FOR THE DEMONSTRATION OF CERTAIN ILLUSIONS OF MOTION

By LEONARD CARMICHAEL, Brown University, and HAROLD SCHLOSBERG, Princeton University

The device here described is of value in the demonstration and in the analytic study of one type of visual illusion. The present apparatus was developed independently by the authors at Princeton University, but we later discovered it to be somewhat similar to one described by Bowditch and Hall in 1880.[1] Our device, however, seems to present certain advantages over the previous apparatus.

The present machine makes it possible to present under controlled and varied conditions, all of those visual illusions that depend upon what has been termed by Thompson, from its similarity to the movement used in cleaning a dish, a *rinsing* motion.[2] A rinsing movement may be described as a movement in which the stimulus-object is revolved in a circle, at a given distance from the center, but maintains a fixed orientation so that it does not rotate about an axis within itself.

Among the factors that it is possible to vary when the device here described is used, beside the actual form and color of the stimulus patterns, are: (*1*) the speed of the rinsing movement; (*2*) the extent of the excursion, *i.e.* the distance of the circle inscribed by the center of the stimulus design from the true center of rotation; and (*3*) the distance of the *O* from the object.

Essentially, the device consists of two sprockets of an equal number of teeth, coupled by means of an endless chain, so that when one sprocket is driven, the other also turns at an equal rate of speed. Thus all corresponding radii of the two sprockets always remain parallel and when a connecting yoke or strip is attached by a pivot to a pair of these corresponding radii, at points equidistant from their respective centers, this yoke will be moved with a circular motion, but not rotated; *i.e.* the motion imparted to the yoke will be the *rinsing* motion previously described.

In the following description the numbers in parentheses following the names of the parts refer to the indicated points in the accompanying cut. The sprockets (1) and chain(2) are placed behind the upright support (3). The yoke (4) is attached to arms (5) mounted on the same shafts (6) as the sprockets, but on the opposite side of the upright support (3). A board (7), made of soft wood, is mounted on the yoke, and to this base the stimulus patterns (8) may be attached by means of thumb tacks.

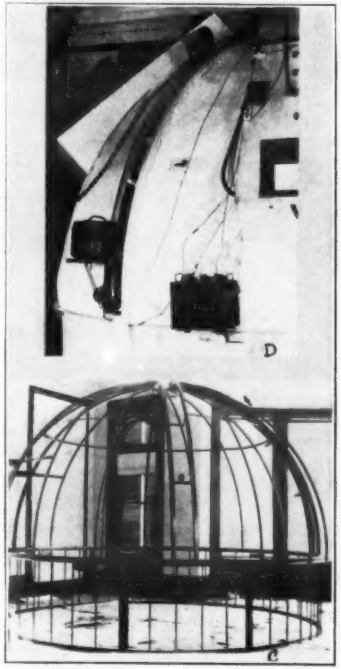
The dimensions of the parts are as follows. The sprockets (1) are 11 cm. in diameter and they have 36 teeth, and they are mounted with set screws upon a steel shaft 2 cm. in diameter and 7 cm. long. Each shaft passes through a brass bearing (9) mounted in the upright support (3). The

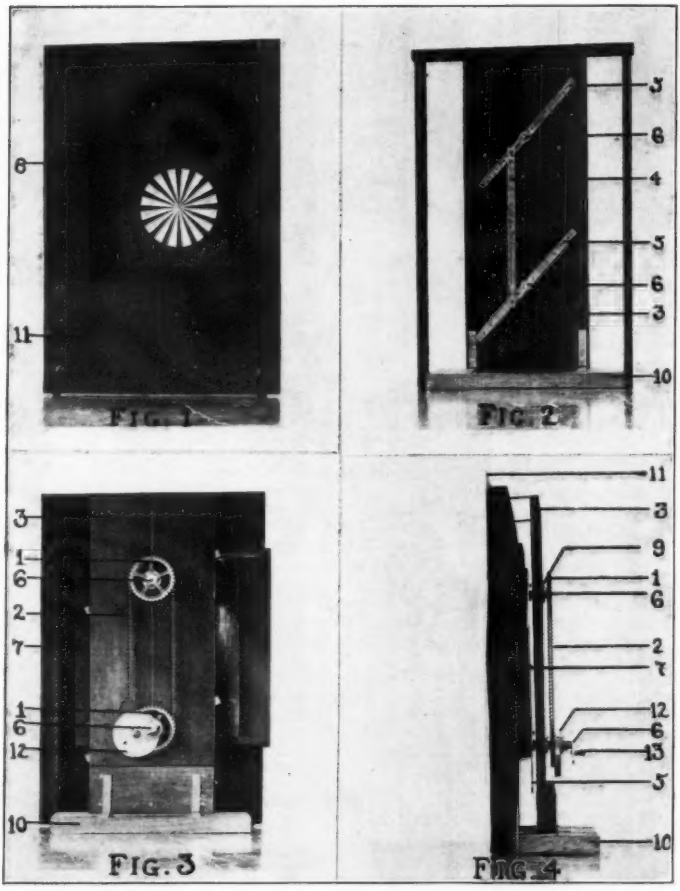
[1] H. P. Bowditch and G. S. Hall, Optical illusions of motion, *J. Physiol.*, 3, 1880-1882, 297-307.

[2] S. P. Thompson, Optical illusions of motion, *Brain*, 3, 1880, 292.

FIG. 1. YALE PSYCHO-CLINIC PHOTOGRAPHIC OBSERVATORY

A and *B* are, respectively, overhead and horizontal simultaneous photographs of an *S*. *C* shows the iron framework of the dome; and *D* is a view of the completed dome showing the left camera track; the still camera mounted on its carriage; the motion camera above on the same track; two boxes containing the lamps; the storage battery; and some of the electrical connections





FOUR VIEWS OF A DEVICE FOR THE DEMONSTRATION OF CERTAIN ILLUSIONS OF MOTION

FIG. 1. Front view, as seen by the class or observer.

FIG. 2. Front view, with the shield and the board with mounted stimulus pattern removed.

FIG. 3. Rear view.

FIG. 4. Side view.

bearings are both in the mid-line of the upright support, and the center of the lower one is 20 cm. from the baseboard (10) and that of the upper is 35 cm. above the lower.

The arms (5) are 34 cm. long and are made of strip brass, 2 cm. wide by 3 mm. thick. They are fastened to the shafts at their middles. Holes are drilled every 2 cm. and tapped. The yoke (4) is made of the same strip brass, and is 39 cm. long. Holes are drilled 2 cm. from each end, and the yoke may be attached to any pair of corresponding holes in the arms.

The upright support (3) is 75 cm. high, 30 cm. wide, and 2 cm. thick. It is mounted on a baseboard (10), 46 by 25 by 3 cm. in size. Angle irons are used to make this support more firm. The board (7), on which the stimulus pattern (8) is fastened, is mounted on the yoke (4). The board itself is 45 cm. square. It is covered with a piece of dull black cardboard of the same size. To cover the moving parts of the device a screen of black cardboard (11) is mounted 5 cm. in front of this board. The screen is 75 cm. high and 50 cm. wide. The aperture in the center of this frame-like screen is 35 cm. square. The complete device as seen by the class or by the *O* is seen in Fig. 1.

To counterbalance the eccentrically moving parts, a lead counterweight (12) is mounted on the lower shaft. If this counterweight is properly adjusted, and if the moving parts are fitted carefully, the apparatus will operate smoothly and quietly. No moving parts are exposed to the observer, except the stimulus field.

The device may be operated by hand by means of a crank (13) or by a small electric motor and reducing gears. A range of 50–400 R.P.M. will be great enough for the demonstration of most of the illusions requiring this type of motion.

No wholly satisfactory analysis has been presented of the various factors important in the production of the illusions dependent upon this rinsing motion. The early theories proposed to account for these phenomena have been well summarized by Bowditch and Hall.[3] Sanford has also considered these illusions and has reprinted certain of the more striking stimulus patterns.[4] We have begun a more complete analysis of the phenomena included in this group of illusions, in which the influence of various colors upon the stimulus patterns will be particularly studied. The device seems to be as satisfactory for this analytic work as it does for the lecture-table demonstration, for which it was primarily developed.

TIMING THE FILMO AUTOMATIC CAMERA

By H. M. Halverson, Yale University

The Filmo camera operates by means of a spring motor which, when released, synchronizes the rotation of the shutter with the movement of the film through the film channel. The camera is standardized to run at two speeds, 16 exposures (normal speed) and 8 exposures per sec., by the adjustment of a large slotted screw which controls the motor governor. One should expect to obtain a speed of 12 exposures by adjusting the screw to a point intermediate to those at which these two speeds are obtained.

We have carefully marked this intermediate point, and timed our camera for all three speeds. For each timing the motor was fully wound

[3] *Op. cit.*
[4] E. C. Sanford, *Course in Experimental Psychology*, 1898, 313-318.

and permitted to run until it came to a stop. A stock camera procured from a local dealer was timed in like manner.

The charted results indicate that the cameras perform at a speed approximating that for which they are designed. The decrease in the speed

Speed Indicator at	Laboratory Camera					Stock Camera				
	Ave Speed	m.v.	Initial Speed	Final Speed	Duration of Run	Ave Speed	m.v.	Initial Speed	Final Speed	Duration of Run
16	15.2	0.4	16.3	14.3	69 sec.	15.7	0.4	16.6	14.7	70 sec.
8	8.7	0.5	9.7	7.7	120 sec.	6.4	1.2	8.0	3.4	167 sec.
	8.9	0.4	9.7	8.0	100 sec.	7.4	0.4	8.0	6.4	100 sec.
12	13.5	0.6	14.7	12.5	77 sec.					

of the motor, if it is permitted to run no longer than 60 to 70 sec., is slow and constant. To secure a speed of 12 exposures the control screw should be set at a point slightly nearer the 8 than the 16 point on the speed dial. The two Filmo motors did not perform exactly alike for any given speed adjustment.

A NOTE ON APPARATUS USED FOR SECURING INSPIRATION-EXPIRATION RATIOS

By Carney Landis, Wesleyan University

Several years ago Nixon described a simple device for recording inspiration-expiration time.[1] As part of an experiment concerning the detection of deception on the basis of the inspiration-expiration ratio I had the device described by Nixon constructed by an expert mechanician. When I endeavored to use it, I found it to be unsatisfactory and ill-adapted for exact experimental work. Slight changes in the position of the subject's body, in his rate or amplitude of respiration were sufficient to cause it to cease functioning. Even under the best of conditions the electrical make and break tongue could not be made to function at anywhere near a constant point in the respiratory wave.

In previous experiments I have made use of the apparatus described by Burtt.[2] This apparatus when properly adjusted followed accurately the respiratory wave, showing a clean make and break at the points of inspiration and expiration, but in practice I found it difficult to keep Burtt's apparatus in proper adjustment. When the adjustment was faulty, however, the fact was clearly indicated by the record—a virtue that Nixon's device does not possess.

In view of these facts it would seem wisest, if one desires the accuracy necessary to make diagnostic use of the inspiration-expiration ratios for the detection of deception, to record respiration directly by means of a pneumograph connected to a recording tambour.

[1] H. K. Nixon, A simple device for recording inspiration-expiration time, *J. Exper. Psychol.*, 6, 1923, 383-386.

[2] H. E. Burtt, A pneumograph for inspiration-expiration ratios, *Psychol. Bull.*, 15, 1918 325-328.

BOOK REVIEWS

Aphasia and Kindred Disorders of Speech. By HENRY HEAD. Cambridge, The University Press; New York, The Macmillan Co., 1926. Vol. I, pp. xvi, 549; Vol. II, pp. xxxiii, 430.

In the history of every science, changing fundamental concepts or philosophical points of view dominate. These concepts affect the development of the science, they retard as well as help. They stimulate studies towards one end, they stifle others which do not conform to the general pattern. But, as development in a science takes place, the current conception becomes like the tomb of the Arab, covered with contributions from the devout, so that the original grave form disappears.

Although aphasia as a scientifically studied field is relatively new there has been time for the development of a number of points of view. These have been partly offshoots of conceptions of cerebral-mental relations, and partly description of theoretical clinical entities. For one who wants to know something also of these varied interpretations, the early chapters of Head's work are indispensable even though they do not pretend to completeness. In this part Head first deals with the early period during the renaissance of medicine when the idea of vital spirits was dominant. That period he relates to the fixed localization doctrine of Gall and his immediate successors. In the succeeding sixty years the accumulation of data culminated in the pronouncement of Broca, which was criticized by Hughlings Jackson, but without general approval. Subsequent "diagram makers," such as Bastian, Kussmaul, Wernicke, and their followers, introduced the concepts of possible clinical relations, which then dominated. Later work, described by Head as "chaos," included the strictly anatomical localizations of Henschen, the opposing views of Von Monakow as illustrated in his hypothesis of diaschesis, the apraxia studies of Liepmann, the language-analysis school, the concept of intellectual disturbance as illustrated in the work of Marie, and the contributions of Pick. Marie, "the iconoclast," stands out almost as prominently as Hughlings Jackson as a prophet or apostle of a new generation. It is to the iconoclast school that Head belongs, but perhaps more to that represented by the English neurologist than that by the Salpetrière professor.

It is with Head's work and views that we are now chiefly concerned. Much of the earlier discussion of aphasia was based on the study of elderly patients, who because of arteriosclerotic changes had brain destructions due to cerebral hemorrhages (apoplexies). In addition to the impossibility of fitting into current schemes the facts noticed in patients of this age, Head's special interest and point of view developed because of his work with more youthful soldiers whose brains had been injured in war accidents. Instead of adhering to the historical methods of examination Head devised and used methods whereby the ability to 'think' could be tested, sometimes without the intervention of obvious verbalization.

The results of the work indicated the necessity of looking at the disturbances in the field of aphasias from a new point of view, which may briefly be described as that of symbolic thinking. This concept of aphasias bears a relation to the views of Hughlings Jackson on imperception, those of Liepmann on apraxia, those of Marie on intellectual defect, and to those of Pick. Head points out (Vol. I, p. 83), what the reviewer has also previously emphasized, that the fluctuations in speech functions are important phenomena, and that the aphasic is not static. One of the cases cited (No. 20) in which recovery was relatively rapid after the removal of a tumor pressing upon the brain substance, "yielded invaluable records illustrating the relation to one another of the various forms of verbal formulation and expression and of the order in which they were restored." In a number of places Head also indicates the importance of the *Gestalt*. He

points out (I, p. 420) that anxiety and worry affect the speech function, and that reactions "vary, not only with the mode of giving the command, but with the situation at the moment" (I, p. 121). "The older views . . . . have given place to the idea of a single unitary reaction in response to multiple factors" (I, p. 133). In other places behavior psychology is prominent, when for example, he says that it is "impossible to find any single term to include all these disorders of behaviour, which extend on the one hand for mechanical aptitudes to exercises in formal logic on the other" (I, p. 217), and which characterize the aphasic states. Speaking, reading and writing are the normal, and "aphasia, amnesia, alexia, agraphia, agnosia, and other terms of a like nature . . . . are convenient shorthand descriptions of varieties of . . . . abnormal behaviour" (I, p. 300). "The various disorders of speech . . . . manifest the ways in which the organism masters a situation . . . . with a defective mechanism" (I, p. 301).

Speech disturbances, grouped under the name aphasia, are, however, described under the formulation "disorders of symbolic thinking and expression." It should be understood, however, that (I, p. 399) "symbolic formulation and expression is not more than an empirical term." Language "consists of a vast number of ways in which an individual meets his imperious desire for expression" (I, p. 301). Even in those aphasic states known as 'jargon' the patient tries to express himself, but we fail to catch his meaning because we cannot follow his associative processes, principally on account of the verbalizations he uses. Frequently his rapid replies are correct, but there are difficulties which are evidenced only by a retardation in responses (I, p. 149). The reaction of the aphasic is frequently only an easy way out of a recognized difficulty. So-called images, especially the visual, may not be disturbed, and memory and intelligence may not be affected (p. 418). The patient who is restricted in his speech expression is thrown upon himself, because he no longer meets the social demands of a dinner party, the newspaper, and the like. A patient may know "what he wants to say, but he cannot discover suitable terms in which to express his thought to himself or to others" (I, p. 402). "Speech is a mental process which we may or may not exteriorize; an aphasic is not only deprived of power to speak aloud, but he has also lost the capacity to formulate his thoughts to himself in certain symbolic terms" (I, p. 400).

The clinical varieties of aphasia which Head believes can be distinguished are (1) verbal, (2) syntactical, (3) nominal, and (4) semantic defects.

The verbal defects are those in which the patient may be limited to a 'yes' or 'no' response, or in which the articulation is imperfect or halting. Comprehension may be slightly lowered, but frequently becomes restored, although silent reading may be difficult because the patient is "compelled to look back at the beginning of a long sentence in order to obtain its full meaning" (I, p. 413). His comprehension in other directions may be good, although he may give an incorrect verbal response. For example, in scoring at card games, he may say the wrong number, but his general behavior is consistent with an understanding of his correct score. The main defect is that of "verbal structure and words as integral parts of a phrase" (I, p. 413).

Jargon is more characteristic of the syntactical defects. Words may be incorrectly enunciated, but there is also a lack of grammatical coherence. Word meanings may be apprehended and reacted to in ways other than verbalization. The aphasic of this type appears to take in the meanings of his silent readings. "The patient has plenty of words, but their production is ataxic, and they are strung together without the usual connecting links. This leads to jargon and renders difficult even internal formulation of words and their meanings" (I, p. 414).

The nominal defects are primarily the inability to use words and to understand their values. The patient may, nevertheless, be able to give

many words relating to the matter he wants to describe. Because of his inability to formulate in characteristic words the things he wants to describe or picture, he may appear to be stupid. He may try to draw the plan of a room, but may leave out essential elements, but put in minor details, and in doing this he may exhibit important parts of the ground plan in elevation. There appears to be a confusion of ideas.

The semantic defects are "characterized by want of recognition of the ultimate significance and intention of wards and phrases, apart from their direct meaning" (I, p. 416). Writing tends to be confused, and drawing is poor. "These patients . . . . fail to recognize landmarks," and, therefore, cannot be trusted to go about alone. "The tendency to confusion and want of comprehension . . . . leads these patients to seek solitude."

Regarding the anatomical correlates of the aphasias, Head concludes (I, p. 441) that "the more definitely the injury destroys the lower portion of the pre- and post-central convolutions and the parts which lie beneath them, the more likely are the defects of speech to assume a verbal form. A lesion in the neighborhood of the upper convolutions of the temporal lobe tends to produce 'syntactical' disorders. Destruction round about the region of the supra-marginal gyrus causes defects in the use of language which I have called 'semantic;' whilst a lesion situated somewhat more posteriorly seems to disturb the power to discover and to understand names or other 'nominal expressions.'"

These statements of neurological localizations must, however, be understood in the light of the author's general view of cerebral functions. He accepts the view of deviation of Sherrington, and he quotes with approbation the reviewer's concept of the cerebral cortex as a labile organ. He considers it a fallacy to conclude that the motor cortex is "the seat of strictly limited 'centers' of preordained function," and he believes that the destruction of a 'center' produces only a transitory loss of function, which may be followed by recovery. "The deeper and wider . . . . the injury to the cortex . . . . the graver and more permanent is likely to be the loss of function; but we must never forget that it has disturbed a highly organized act and has not removed a strictly anatomical 'center'" (I, p. 436). "A complete act of speech comes to be a wide-spread response of the organism to each fresh situation . . . . Since the cortex is a more flexible organ with less rigid and preordained reactions, the disturbance of function due to injury of the surface is not so severe and permanent and is less easily determined . . . . All attempts shortly to differentiate a cortical from a sub-cortical aphasia are bound to fail" (I, p. 441). There are "no 'centers' for speaking, reading, writing or other forms of behaviour comprised in the normal use of language" (I, p. 478), but there are areas concerned with symbolic formulation and expression in a single hemisphere. Injury to different parts of the brain may produce somewhat different speech diturbances. On the other hand, we must understand that (I, p. 499) "no function is 'localized' strictly in any part of the cortex and no form of activity, somatic or psychical, is built up into a mosaic of elementary processes which become evident when it is disturbed by a lesion of the brain."

No brief summary such as this can give an adequate view of the many important observations and of the acute analyses of the speech disorders contained in the two volumes. Head has introduced into the aphasia problem a new point of view, and his methods of examination have brought to light conditions which would not have been suspected through the use of the older examination methods. More important than the new methods, and more important than the new classification is the turning away from the static picture of the aphasias and the insistence on a dynamic concept. Speech, whether normal or abnormal, is always dynamic. It is always a changing form of behavior, an adaptation to the ever-changing present situation. Moreover, none of the aphasias is self-contained. The verbal

may give place to a syntactical or to a semantic type, and there may be in one patient evidence of two or more forms. Although Head does not insist as much as the reviewer has done upon the importance of the phenomena in reëducation, this has obviously appealed to him. It is, therefore, the whole picture—the defect and the recovery from the defect—which must be looked at if one is to have an adequate view of the aphasias. It is the redevelopment of the speech function, or the reëducation of the aphasic, which the reviewer believes will give more adequately facts respecting the re'ationships of speech defects and the relations of the brain to the speech function. As of special value to the teacher in relation to student instruction we may accept the general formulation that the precentral cortex is the controlling mechanism for voluntary movements. But that formulation can not explain the phenomena of recovery of voluntary control after destruction of the so-called motor cortex, nor the other facts of disturbances in voluntary movements after destruction of other brain parts. The control of movement is a diffuse neurological control. It depends upon the combined actions of a number of brain parts. So also the speech functions depends upon the combined actions of many cerebral (cortical and sub-cortical) elements.

The conditions upon which the speech functions depend are relatively unknown. Why an aphasic may be able to sing, not only tunes but also accompanying words, but be apparently unable to use the same words in conversation or at command, is a scientific mystery. Why he may be able to describe a "key" as "an object which goes into a lock," but be unable to give the name "lock" is another scientific mystery. Why he may be able to give the name "lock" on one day every time the object or the word may be shown to him, but on the succeeding day be unable to give the word at any time is a third mystery. Other similar mysteries could be cited.

If one has a faith, or an intense belief, they may not be considered to be mysteries. If, however, one accepts and defends a general hypothesis, it frequently happens that the acceptance leads to more and more tenuous guesses. The fabric changes from opacity through translucency, to a transparency, in which latter state fact or science disappears. There is a tendency to look through the opacity and 'see' all things revealed. The translucent shows only what is expected or suggested. It is, however, not difficult to get the initiated into the frame of mind that he will "hallucinate" with the transparent. In science we try to get the transparent state, but without the dominating faith.

Head has taken the aphasias out of the opaque screened darkness and has brought them into a position in which we can see them, if we will, in better outline even though it may be vaguely. He has brought them from the limbo of faith, creed, or scientific belief, into the light of experimental investigation. There remains for the future the tedious and the protracted task of lighting them still more.

The reviewer's studies of aphasia do not make him see Head's work and conclusions through blue or through rose-colored glasses. He thinks a more general formulation of the aphasias may be made, but he also sees that Head has made a most notable contribution, as notable as, and perhaps more notable than, his previous studies on the sensations of the internal organs, the sensory functions of the peripheral nerves, the spinal cord, the thalamus, and the cerebral cortex.

The inclusion of cases 23 (II, pp. 348 ff.) and 25 (II, pp. 379 ff.) is unfortunate. These cases are those of so-called congenital alexia and agraphia, or congenital word-blindness. They are disorders of the acquisition of speech, and not disorders of acquired speech. Persons showing these disturbances are cases who do not acquire the abilities to read and to write mainly through vision, as most children and adults do, but by kinesthesis. They have been intensively studied by G. M. Fernald who with special procedures has been able to get these patients to acquire

the abilities they could not acquire in the more usual manner. They do not acquire some speech symbols in the ordinary fashion, and because of special social (mainly school) conventions they fail to make certain adjustments. That they are not incapable of making the adjustments is evidenced by the fact that when they are handled in a suitable manner they learn to read and to write readily and sometimes with greater facility than the normal.

The reviewer would be willing to make a generalization broader than that of Head. Now that Head has shown the inadequacy of the "diagram makers" and of other schematists, it would be well to get away from all classification for a time. From the point of view of both neurology and psychology it would be well to settle upon a more general formula, such as "difficulty or defect in the formation of associations," as the basic phenomenon in aphasia. If this fundamental formulation were adopted it would stimulate to many constructive studies of the diversities and the degrees of difficulties. A more specialized formulation is a stimulus to either blind acceptance and support, or to unreasoned opposition. And this has been the history of the aphasia problem.

Notwithstanding these criticisms, the contributions of Head are the most valuable which have appeared in several decades. If they are carefully studied they are bound to rid both neurologists and psychologists of traditional explanations and of superstitious beliefs. It is because of this that the work is truly monumental, partly self-erected, and largely erected for others who travel the aphasia road who may see its device, "Excelsior."

University of California, Los Angeles SHEPHERD IVORY FRANZ

The Trend of History: Origins of 20th Century Problems. By WILLIAM K. WALLACE, New York, The Macmillan Co., 1922. Pp. 372.

The author sets out on a relatively new type of interpretation in regard to history. His key is furnished in the preface where he says, "history is the book of life of mankind." He points out that it ought to be more than a mere record of events and should inculcate the public spirit of the times that brought about gradual changes in the different forms of government. He expresses as the purpose of the volume, "to trace through the tangled maze we call the course of events the logical antecedents and coefficients of, . . . the process of social life."

The volume has considerable interest for the social psychologist since at every epoch the author emphasizes the state of mind which brought about transformations in the underlying theory of the State. For instance, very early in the exposition we find a religious influence contrasted with the political consciousness: "In contrast with the religious instinct common to mankind, we find among civilized people what we may term political consciousness" (p. 3). Later on, he shows that in what he calls the politico-theistic concept of the State the Church was distinct from the State in that "secular laws were not binding on the clergy." In this form of government there was a close analogy between God as a supreme ruler of mankind and the nobility who ruled the state. An inseparable connection appeared between the characteristics which were ascribed to God and those attributed to the nobility.

This was superseded by the politico-juridic concept which claimed that the personality of the individual should be projected into the State and the action of the State was therefore to be made amenable to the will of the individual. When this idea occurred to the social mind of those governed, the notion was inculcated that the State must be moral. This lead to a constitutional form of government in which the rights of the individual were codified, and later developed into the representative type of government when the concept of humanity was instilled into the public mind.

Still later there occurred a rise in economic interests together with a development of nationalism with more emphasis on the welfare of the middle classes. About the middle of the last century, too, communism showed its power in the State. All these influences indicated the tremendous problem that the pursuit of wealth was raising in human affairs. At the same time scientific investigations gave men greater confidence in their own accomplishments. Curiously enough, this was coupled with the consciousness that man was left "isolated in an evergrowing, everchanging world" (216). All of this came to a head since 1870 in terms of a politico-economic concept of the State. The thesis here was that the "State exists merely as a means for promoting the welfare of the individual" (223). Gradually, through the rise and fall of imperialism and the gradual increase in power of the proletariat, new concepts began to show their influence, including what the author calls "the substitution of quantitative for qualitative standards" (p. 258). This factor is traced through the remaining chapters in international politics as well as in the development of culture throughout the population of a Super-State.

Thus far the impression may have been gained that the book is full of theoretical discussions, but it remains to be indicated that at every turn the author brings to bear his scholarly knowledge of historical and philosophical tendencies. Historical events together with dates dot the pages of the work wherever the author wishes to substantiate the principles which he has laid down. The volume ends with what appears to the reviewer not altogether an optimistic turn, in that the State now emerges as Power (346). We wish that the writer with so much historical background could have functioned, also, as a prophet and could have told us something of the possibilities for the future. He leaves us buried in thought as he closes his volume with the statement "the State had become the shell, it was no longer the kernal of social life."

State University of Iowa CHRISTIAN A. RUCKMICK

Psychology and History. By HARRY ELMER BARNES. New York Century Co., 1925. Pp. 195.

The present volume is a reprint as a separate work of the chapter on "Some Phases of the Significance of Psychology in the Equipment of the Historian" in the author's *New History and the Social Studies.* It was deemed advisable to make this particular portion of the larger work more readily accessible to psychologists and psychiatrists who would have a special interest in the subject and who would want to know of the efforts which have been made by historians and others to apply psychological methods and data to the interpretation of the history of man and human culture.

The book presents an excellent summary of writers on psychology whose views have been of significance to the student of social science—such writers as James, Hall, Baldwin, Thorndike, McDougall, Trotter, Watson, Freud, Cattell, Bagehot, Le Bon, Tarde, Wundt, Boas, Lamprecht, and Robinson. The psychologist, however, will find the summary of little value to him except in so far as it offers suggestions for points of departure into fields with which he is relatively unfamiliar. The review, of necessity, is too brief and selective to give more than a vague apperceptive background for psychology in relation to social science. The psychological points of view which Professor Barnes selects for treatment are too near the frontier to be accepted or rejected upon cursory examination of them in epitomized form. Few, if any, of them have as yet become shaken down into the generally accepted body of psychological doctrine. They are, in other words, hypotheses, and, unfortunately, hypotheses which have for the most part only dialectic rather than factual support. To seek for the springs of human action and the motivation of conduct means that one

must enter a domain which is marked by a plethora of speculation and argument and a paucity of observational data. But it is just this problem of motivation which the psychologists are asked to solve by those who are interested in the social sciences or in practical affairs. The students of intellectual history, judging from Professor Barnes' book, are no exception to this rule. To them the book ought to be an invaluable guide to modern trends of psychology in the field of motivation. The author has brought together in concise and clear form a vast array of material. One notes only occasional lacunae: the work of men like Rignano, studies in animal behavior, and some evaluation of the question of hedonism would seem to be pertinent to the general purpose of the book.

The psychologist will be more interested to see in what way the historian tries to make use of certain portions of psychology. A section on "The Psychological Interpretation of History" treats of this problem. Following the lead taken by such men as Lamprecht and J. H. Robinson the present author makes it clear that the study of intellectual history needs some sort of *dynamic* psychology as a guiding principle in its interpretation of cultural movements of the past. The author does not state exactly what he means by dynamic psychology—any more, for that matter, than do the psychologists themselves who use the term. It would seem that a dynamic psychology is a sort of *faute de mieux* either for complete analysis or for an analysis in which the parts separated out descriptively fail to present an adequate picture of some driving, purposeful aspect which seems to attach to the whole. Be that as it may, dynamic psychology in the present context seems to imply the Freudian type of psychology principally. By an adaptation of the Freudian analytic procedures to leading personalities and to institutional and cultural history a wealth of new material will be presented to the historian for study and examination.

The reviewer comes away from the book with certain doubts and misgivings. In the first place the question arises as to the wisdom on the part of historians of taking over for their own purposes a methodological tool which is admittedly imperfect and rough in the restricted tasks for which it was chiefly designed. Motivation, instinct, drive, unconscious, purpose: these are labels which one would have difficulty in sticking on the right object. They are hardly concepts which have yet been clearly and univocally enough worked out to warrant the assumption that psychology has hit upon a marvellous methodological 'open-sesame' even within its own field of research. There are passages in which Professor Barnes appears aware of this difficulty (*e.g.* p. 151 ff.), but on the whole his thesis is an enthusiastic and ardent plea to his colleagues to revolutionize the study of history by the application of dynamic psychology. It is another case of a premature application of a 'new' psychology in the hope that 'scientific' miracles may be worked in social studies and the world of affairs. This premature technological bent is further apparent in the author's general attitude towards science. For Professor Barnes science seems to be a sort of magic tool by which man is to pattern the world after heart's desire, rather than a refinement of observational and experimental technique by which he may hope to discover uniform relations which exist between things *just as they are*. Passages such as the following are numerous: "Without bravely facing these realities which demonstrate the 'sickness of the modern acquisitive society,' however, we cannot hope to improve our social order. . . . Until something like the same degree of objectivity can be produced in the social sciences that exist today in natural science, we are likely to continue with our common-sense and rule-of-thumb methods in economics, politics, and modes of social conduct, a procedure which is becoming yearly more dangerous and menacing with the increasing complexity of modern life. If this is an impossible aspiration, then the 'jig is up' with the human race. . . ."

Finally, it strikes the reviewer that the author has at times tried to force into one universe of discourse items and data which are not intimately related. Let us assume for the moment that dynamic psychology offers the historian a means of analysing the motives and characters of leaders of history. Does it follow that, because one knows the motives and subtle circumstances and influences which led some political leader to adopt or work out a certain type of social philosophy, one is thereby in a better position to understand the philosophy itself or its relation to subsequent events? It has been said that the only sure way to understand philosophy is to discover the factors which led a given thinker to cherish certain views and scorn others; that the only way to understand Schopenhauer's philosophy, *e.g.* is to know the conditions in Schopenhauer's life which made a certain *Weltanschauung* pleasing and acceptable to him. But is not Schopenhauer's philosophy and its relation to previous and subsequent systems a subject-matter to be analysed in its own right without relation to the personal motivations of the philosopher himself? Does it aid one in 'interpreting' intellectual history to know, as the author states, that Augustine's theology was a psychic compensation for a wild youth and varied and extensive sexual experiences, or that Alexander Hamilton's love for authority centralized in the government was due to the absence of parental control and authority in his early home life?

One is dealing here with two fields of inquiry which are not very closely related. No one will deny that there is a connection between them, but to follow this connection through its ramifications would lead one into a bewildering regress of causes which would throw very little light, even if they could be accurately determined, on questions of personal biography or matters of history proper. It would seem particularly advisable in the social sciences to mark off the various fields of investigation fairly sharply in order to bring restricted sets of phenomena under the application of suitable methods. History has its own method; biography has had another method, and is perhaps the sort of subject which could profit considerably by the use of recent methods to study the motivation of behavior. But it does not necessarily follow that, because a certain individual has played an important part in some phase of the world's history, one will have a larger knowledge of that particular portion of history by studying the physiological and psychological traits of the individual. For purposes of historical study the ideas and actions of an individual form one set of phenomena to be examined in a restricted context. The same phenomena in another context constitute the starting point for psychological and physiological investigations. The relation of the motivation of an intellectual process within a given individual's psychophysiological constitution to the subsequent historical fate of that same process when presented to the world in systematic form is one which is more likely to be 'interpreted' than observed.

Harvard University C. C. PRATT

Tierpsychologie vom Standpunkte des Biologen. By FRIEDRICH HEMPELMANN. Leipzig, Akademische Verlagsgesellschaft, 1926. Pp. viii, 676.

This work by the professor of zoölogy and comparative anatomy at Leipzig is divided into a *spezieller Teil* of 454 pages, an *allgemeiner Teil* of 182 pages, and an extensive bibliography of 26 pages of small type. The first division treats the behavior of the various phyla with the following allotment of pages: Protozoa, 74 pages; Porifera, 2; Coelenterata, 29; Echinodermata, 23; Vermes, 42; Mollusca, 23; Arthropoda, 120, including 54 on Insecta; Vertebrata, 119. The second division has chapters on Special Capacities (vision, orientation and homing, time perception, counting-ability, childhood, play), Form a Stimulus (reaction to form-

stimuli, perception or relation and *Strukturfunktion*, the *Gestalt*-principle, ideas), Feeling Life of Animals (feelings and emotions, speech, ethical and aesthetic feelings), Analogies to Special Psychic States in Humans (sleep, dreams, hypnosis, mental diseases), The Brain as Organ of the Psyche, The Psyche, and History and Methods of Animal Psychology.

The author's purpose is to furnish a comprehensive summary of the established content of animal psychology, avoiding so far as possible too great emphasis either on physiological processes "or on the ethology or ecology of animals." More specifically, the plan of the work seems to be to give full accounts of the behavior-repertories of the various phyla in the first part, and to supplement these accounts with a few chapters of general import in the second part. The book is therefore composed primarily of facts, most of which were experimentally obtained, rather than of theories; and no attempt is made to discuss the anatomy and physiology of receptors and nervous systems except in a few places where the treatment is concise, general, and yet valuable.

The standpoint, indicated in the title, is that of the biologist rather than that of the psychologist; for this reason psychologists will fail to find any particular emphasis laid on a number of problems which have occupied them, especially in America, in recent years. For example, scant attention is paid to maturation *vs.* learning in 'instinct' (no mention is made of Breed and Shepard's work on chicks, or of Yerkes and Bloomfield's on kittens), to the delayed reaction (there is no mention of Hunter's work), or to the order of elimination of errors in maze learning, etc. There is no account of the recent discussions as to the rôle of insight in animal learning, such as would be presented if Koffka's criticisms of Thorndike were summarized; the author goes no further than to give Köhler's statement, with rather extensive quotation, that the learning of apes is 'insightful.' The more purely biological and older problem of the tropism is, however, adequately treated; various criticisms of the Loeb doctrine by Von Buddenbrock and others are quoted.

The account of Protozoa is quite full and includes the theories of amoeboid movement and a discussion of the artificial amoebae of Büschli and others.

The section on insects consists of individual rather than social psychology; only three pages are devoted to social insects. This omission is not to be deplored, as the field is well covered by such books as Wheeler's. The main topics treated are: the rôle of the senses in insect behavior, reflexes, and instincts. The author insists throughout that insect behavior is not wholly rigid but is modifiable, and he cites experimental proofs. He follows Ziegler in his definition of instinct.

The section on mammals is subdivided into two parts on the various groups, which begin with marsupials and end with apes. This section is somewhat marred by the uneven quality of the material presented; for example, after Pavlov's work is described, the "famous poodle of Lubbock (1889)" is mentioned, and there is a good deal of material, especially on the horse, which is non-experimental. The positive results of Kalischer on *absolutes Gehör* in dogs are given on page 423, while Johnson's negative results in the same experiment are not mentioned until page 432, and the relation of the two experiments is not pointed out.

In the *allgemeiner Teil*, the section on Special Capacities is comprehensive and valuable. In subsequent sections we find a statement of the *Gestalt*-principle, but no criticisms of it are quoted. Under "ethics" and "aesthetics" are discussed sex life, treatment of other animals and of young, "marriage," dances, etc.; under "mental diseases," peculiarities of behavior caused by lesions. The Brain as Organ of the Psyche (including discussion of spinal and decerebrate animals) is too short to be of great value, and the same may be said of the History, which is a mere sketch of

five pages. The author follows Ziegler in his history and appears to think that the latter has furnished such an adequate one that further efforts at this time are not called for. The Psyche is a chapter summarizing opinions on problems such as the criteria of consciousness, the necessity of postulating an entelechy, and the question whether mind appears suddenly in the evolutionary series or is to be assigned to all living organisms.

The author has succeeded remarkably well in his task of assembling an enormous mass of data from many scattered sources. In a work of this magnitude some omissions and neglects are inevitable, and in the present case some of these faults detract from the value of the book. There is, for example, no mention of Lashley's extensive work on cortical function. That this omission is not due to an intended restriction of subject-matter is shown by citation of work on the very same problem as Lashley's on page 405 (Jellinek and Koppanyi's work on rats, 1923). Watson's *Behavior* is cited neither in the bibliography nor elsewhere, but Watson's monographs are adequately treated in the text. Conradi's work on the social influence in the song of birds is not mentioned, although several pages are devoted to song. Some American studies have not been utilized in discussion of form as stimulus (Lashley, Bingham, Casteel).

The book is beautiful in binding, paper, and typography. There are 134 illustrations and a plate of eight chimpanzee expressions. The only serious typographical error is the changing of "Yoakum" into "Choakum." An amusing error occurs when the author speaks of the experiments on Paramecium made by "den beiden Damen Day und Bentley."

Harvard University FRANK A. PATTIE

The Measurement of Intelligence. By E. L. THORNDIKE, E. O. BREGMAN, M. V. COBB, ELLA WOODYARD and the Staff of the Division of Psychology of the Institute of Educational Research of Teachers College, Columbia University. New York, Teachers College, Columbia University, 1926. Pp. xxvi, 616.

This book represents the results of three years of research by Professor Thorndike and his associates at Teachers College on the nature, meaning, and measurement of intelligence. Much of interest and of value to psychologists will be found in it on all three of these problems; but without doubt the most important contribution of the authors lies in the field of measurement, and more specifically in the construction and validation of a scale for measuring a given "segment" of "intellect," known briefly as *CAVD*.

Intellect *CAVD* is strictly defined by an inventory of tasks involving the completion of sentences (*C*), arithmetic problems (*A*), vocabulary tests (*V*), and directions (*D*), the last named often requiring comprehension of sentences and paragraphs. Forty composite tasks—10 each of *C*, *A*, *V*, and *D*—are placed at levels of difficulty ranging progressively from A to Q; or, differently expressed, from a composite (A) with which an adult of approximately 3 years mental age can succeed (get 50% correct) to one (Q) with which only about one-fifth of college graduates succeed. Each of these difficulty levels has a numerical value which represents its distance from an approximately absolute zero of intellect. This zero point was located by utilizing a consensus of judgments, with respect to certain tasks, made by a group of psychologists who were specialists in animal and child psychology. Every judge ranked 56 tasks in an order of merit in accordance with the degree of intellect (in his opinion) required to perform it. From the tasks thus chosen to be of minimal intellectual difficulty, that is to say, at the zero point of intellect, the distance of level A was measured in terms of the P.E. of 'expert opinion.' Illustrations of tasks of zero intellectual difficulty are, "Will not try to pull off his own fingers or toes," "Having an object of bitter nasty taste in his mouth will spit it out more often than hold it."

Three measures of intellect are obtainable from the *CAVD* examination: altitude (or level), width, and area. The altitude or level score locates an individual on a scale of intellectual difficulty graduated from zero as the point of reference. The width score (defined as the percentage of successes at one or more levels) gives the scope of intellect *CAVD* at given difficulty levels. The score for area (defined as the total number of tasks performed) measures the range of intellect *CAVD* throughout the scale. The curve for age with altitude *CAVD* is parabolic and rises from 0 to about 20 years, becoming approximately level at that point. But after 20 years, in Thorndike's view, intellectual improvement may still take place in specialized fields of endeavor (p. 468).

Many findings of first-rate importance in mental measurement and theory must be passed over lightly. Several which should be at least mentioned, however, are (1) a method whereby scores on stock intelligence examinations can be transformed into a scale with equal units (Chapter VII); (2) evidence to indicate that intelligence is almost certainly normally distributed in children and is probably slightly skewed (positively) among adults due to differential gains after 14 years (Chapter VIII and Appendix III); (3) the proposal of a 'quantity theory' of intelligence, namely, that good and poor intellects differ not qualitatively but quantitatively, which is to say in respect of the number of connections which they possess. This view grew out of the experimental finding that correlations between 'higher' abilities, *e.g.* difficult completions, arithmetic problems, analogies, and 'lower' abilities, *e.g.* vocabulary, information, simple arithmetic, are as high as the intercorrelations within either group. This result would seem to suggest that there is a general intellectual level at which an individual habitually functions, and lends force to the recent friendly criticism by Professor Spearman (*Brit. J. Psychol.* 17, 1926, 365) that Thorndike has really stated his theory of the 'g' factor without, it may be added, subscribing to it.

Several criticims of a somewhat different sort will suggest themselves as one reads the book carefully. The style is often obscure and sometimes involved, and the frequent omission of proofs and illustrations does not serve to make, the argument any easier to follow. One will be struck, too, by the frequent need for judgment for deciding what value to take or what procedure to follow, and will wonder whether the *CAVD* scale would remain fundamentally unchanged if the steps in its construction were repeated by another investigator. Again, the almost blanket use of the Spearman formula for correcting for 'attenuation' will seem to many to require justification. Probably no one knows better than Thorndike, however, the very definite assumptions upon which this formula rests; and at least one must admire his scientific honesty in printing all of his corrected r's even when they reach the astonishing height of 1.40.

To be sure, objections such as these weigh lightly against the body of solid achievement in this extensive research. The importance of the *CAVD* scale lies not so much in its extension of the scale for measuring intellect accurately far beyond that of any other scale, nor in the fact that it permits for the first time the "times as much" statement of intellect from an absolute zero. Its real value lies in the method here laid down whereby other scales for measuring traits and abilities, as well as intelligence, may be constructed. Future research may, and probably will, change the values of certain levels and shift the zero point up or down. This is of relatively small importance. The fact remains that Professor Thorndike has enormously clarified and extended our knowledge of mental measurement; and for this achievement, he is deserving of the thanks of all students of quantitative psychology.

Columbia University H. E. Garrett

Social Psychology. By F. H. ALLPORT. Boston, Houghton Mifflin Co., 1924. Pp. xiv, 453.

This book represents an able and interesting attempt to write a social psychology in mechanistic and behavioristic terms. Social psychology is defined as the science of social behavior (*i.e.* behavior which is socially stimulating or stimulated) and the social consciousness of the individual. In view of the behavioristic tone of the whole, the inclusion of consciousness in the subject-matter may prove a trifle surprising; and, in view of the author's belief that consciousness may accompany the mechanistic sequence of stimulation, neural transmission and response (but is never a causal link), his emphasis on it may appear gratuitous. But it is difficult to see how the facts of social psychology can be described in even moderately adequate fashion without reference to consciousness and introspective evidence, and, if, in reading the chapters on response to social stimuli and social attitude, we sometimes forget the epiphenomenal character ascribed to consciousness in the earlier chapters, that is perhaps the fault of our bias. The term "social consciousness" is not used in the sense traditional in this field, but here denotes individual consciousness accompanying the social attitudes.

The various group or collective-mind theories are, rightly enough, discarded at the outset. The only behavior and consciousness to be studied are that of the individual. The author carries out his study of social phenomena without feeling any lack because of discarding these theories. It is perhaps true that the last chapter, "Social Behavior in Relation to Society," which deals with that group of phenomena in explanation of which the hypothesis of the group mind is usually invoked, is weaker than the rest of the book; but may we be so bold as to say that this failing is not due to the lack of such an hypothesis. The author is content in that chapter to deal with his subject rather in the fashion of a sociologist who dabbles in psychology and economics than as a psychologist who applies his principles with discernment to the larger and more interesting group phenomena.

Instincts are also discarded, but less successfully than the group mind. Instead of instincts we find prepotent reflexes, which are also drives and demands. The chief advantages of prepotent reflexes over instincts are that they are fewer, simpler (since they involve only the simplest stimulation and crudest movements in response), and, save for the sex reactions, are all manifested shortly after birth and therefore do not involve the difficulties of a maturation theory. The prepotent reflexes posited by the present author are six: starting and withdrawing, rejecting, struggling, hunger reactions, sensitive zone reactions and sex reactions. From these as raw material the whole story of man's activity is derived by the principles of conditioning and trial-and-error learning.

It is evident that much that is considered instinctive and fundamental by other writers must on this plan be derived from these six prepotent reflexes. Thus, we are told that curiosity, hunting, hoarding and constructiveness are all derived from the tendency to manipulate and that in turn from the mouthing reflex (hunger). This is obviously a rather difficult development to trace and the author dismisses the matter in a few sentences.

When we come to what other writers speak of as the social instinct we find here only social habits. Reactions to social stimuli are differentiated from those to non-social by learning. The "universal tendency to produce reactions in others," that is, social behavior which is sought as an end in itself, is derived from the habit, developed in early childhood, of controlling parents to secure satisfaction of bodily needs; the mechanism is the "hurt cry." The force of habit and the desire to control others, then, form the basis of the whole social side of life. Difficult as it may be for some of us to accept this in regard to the facts of human life, it becomes nearly impossible

to credit it when we turn to the animals. How does it happen that—given in each case a set of prepotent reflexes which contain no fundamental social element—the dog develops a "tendency to produce reactions in others," yes, even in his human master, and the cat lives all her life in sovereign indifference to such low diversions? Defining social behavior, as the author does, in such a way as to include the stalking of a mouse by a cat (linear social behavior) obscures a distinction that is absolutely vital in the study of social psychology. We know that some animals combine with others for various purposes—such as hunting, self-defense, migration, and even for purely social dancing, singing and games—while others, save for a brief mating season, lead highly individualized and solitary lives. Social psychology must provide some principle for the explanation of this difference before it can adequately cope with the facts of *social* behavior. It is highly doubtful whether it is possible to derive a satisfactory description of social behavior from any elements—be they instincts, prepotent reflexes or drives—that do not contain some natively social factor.

This difficulty is brought out in the discussion of McDougall's principle of primitive passive sympathy. The facts covered by this principle are partly explained by the circular responses, which involve one's having experienced an emotion while observing the expression of it in others in order that the emotion be awakened in us by its expression at a later date; but the author notes also the operation of what he calls social facilitation, which he cannot explain. Nor can this be explained on any theory that does not involve some social factor—call it instinct or what you will.

The limits of a review prevent the discussion of all the questions of interest raised in the book. Mention should be made, however, of the chapters on personality, which show the influence of the recent work in that field, and of the very interesting chapter on the development of language and gesture.

Harvard University E. C. GILBERT

Die Lehre vom Raumsinn des Auges. By F. B. HOFMANN. Berlin, Julius Springer. Vol. I, 1920, pp. 213; Vol. II, 1925, pp. 214-667.

Physiologists and psychologists the world over will regret the untimely death last spring of the author of this and other works of interest to them both, primarily in vision. Professor Hofmann had achieved an enviable reputation for scholarly work and careful observation in his contributions to Tigerstedt's *Handbuch der physiologischen Methodik.* Few writers possessed to such an eminent degree his ability to select and coordinate facts from the literature at large, presenting a coherent picture of the present state of knowledge about the subject in hand. With this power went the capacity for painstaking observations which illumine his texts. It is these little experiments that enliven and entice the reader when the text would otherwise be tedious, if not dreary, for Professor Hofmann collated and wrote in true German fashion. The two volumes we have at hand are separate editions of the first and second parts of the 13th chapter of the *Handbuch der Augenheilkunde.*

Since a detailed review of a work like this is impossible, we shall have to confine ourselves to a general discussion of the scope of the work, the style of the author, and his contributions to fact and theory. Certainly the title does not belie the contents of the book: every conceivable factor that should be treated under the perception of space has either a separate chapter or paragraph devoted to it or is given due mention, In fact, the word *Lehre* in the title is slightly misleading if we translate it as 'theory' because the book is full of facts and contains less reference to theory than we might expect. It embodies the known facts concerning spatial perception and if *Lehre* is translated the "teachings" we have an accurate designation for the work.

We have said that the important facts from the whole literature of space perception have been brought together and discussed. It is done not only by the printed word but also by giving numerical results, curves embodying experimental work, and numerous plates and drawings. Opposing views and facts pointing in different directions are quoted in their proper places so that the reader will get an idea of the 'ins and outs' of the problem under discussion. Often when no apparent solution emerges from what would otherwise be a welter of fact, the author points the way by suggesting some theory from the literature, or some fact from his own work, or the type of investigation needed to bring order out of chaos. What Professor Hofmann's industry has accomplished may be gleaned from the fact that his bibliographies contain over 2000 references to books, monographs, and articles. So complete is this work that those who wish to deal with any of the many problems comprehended under the perception, *e.g.* the illusions, visual acuity, the perception of form and *Gestalten*, localization, filling in of the blind spot, correspondence of retinal fields, can neglect this work only if they have mastered the sources from which this author has drawn so plentifully.

In his treatment of the subject the author has assumed that the reader possess some knowledge of the basic sciences, including of course, mathematics. This is a sign of the healthy condition of physiology and points the way to the better psychology. Since the treatment is so detailed one wishes that the author had assumed a litle less on the part of the average graduate student and given him facts (from physics and physiology) which bother him so often. For example, in discussing irradiation and its physical, retinal, and psychophysical causes, the student might well assume that even though the retinal and perceptual field do not have sharp boundaries, the physical source (say a narrow slit) may have, whereas, as everyone knows who has worked with the tiniest light source in a dark room, and in accordance with Huygens' principle, we cannot take for granted the physical boundaries of a visual stimulus, sharp as they may appear against the surrounding field. A word of caution here and there from the author on just such points would have been of great help to those most likely to use the book. In such a work as this, too, the need of theory is felt on every hand: to coördinate facts, to bring the one out of the many, and to establish principles that are the beacons of science. It is here that the reviewer senses the greatest shortcomings of the book.

Remembering that these volumes form part of a larger work, *Physiologische Optik*, the psychologist will naturally be interested in the contribution which Professor Hofmann as a physiologist and which physiology as a science have to make to the psychological problems involved. It is at this point that the discriminating reader will be at least a little disappointed. For where we might expect some enlightening physiological hypothesis or fact to clear up a situation, we find the usual references to attention and past experiences as the maids of all work. Although we have good reason to expect the physiologist above all others to be weary of such easy psychological explanations, we too often find him resorting to these and supposedly psychological mechanisms to fill the gaps in his physiological knowledge. Thus Hofmann accepts Hering's explanation of the great distance between the 50 and 100% points in the perception of difference in position as due to attention; since attention is not devoted to small differences in the retinal images, they pass unnoticed (vol. I, p. 61). This explanation is also supposed to hold for the fact that the perception of small differences is lowered with practice. If one began to ask questions of the facts supposed to be explained by the mysterious activity of attention, which can do most anything, and Übung, he would find the most interesting set of problems of any proposed by the author. The psychologist will therefore find no startling hypotheses (which, perhaps, should not be expected in a *Handbuch*) and certainly he will find

many of the hackneyed explanations of the psychological texts when the author trenches upon psychological material.

Die Lehre vom Raumsinn is recommended to those who, wishing some orientation through the intricate maze of factual material of the subject, have mastered their German sufficiently well to read a rather difficult and condensed style and one abounding in the technical terminology in which the subject is enmeshed.

University of Kansas HARRY HELSON

Vom Fühlen, Wollen und Denken. By THEODOR LIPPS. Third revised edition, Leipzig, J. A. Barth, 1926. Pp. vii, 275.

A revision of the 1902 edition, but so different that the author regards it as a new book, the chief departure being indicated by the sub-title, "*Versuch einer Theorie des Willens.*" This turns out, in fact, to be an extension of his theory of feeling to cover the data of volition. He begins, as usual, with his analysis of feeling.

Common speech often refers to feeling as objective, while psychology, on the contrary, treats warmth and cold, etc. as *empfunden*, while pleasure and displeasure are *gefühlt* as subjective experiences. What is the ground of this distinction between the subjectivity of feeling, and the objectivity of sensation? It is not that feelings are different from sensations in their *quale*, because the sensations are also qualitatively different from each other. Lipps thinks that the difference lies in the consideration that the feelings are *Ich-qualitäten*, that is, that in feeling we literally 'feel ourselves,' just as in sense experience, we sense a not-self. The objectivity of sensations and the subjectivity of feeling are not inferred—they are *lived*. The *ich* is not given us, we live it, *i.e.* we live the "qualitäten des ich." This view comes to the formula, "I feel, therefore I am," for since a feeling undoubtedly exists when we have it, so must the self exist also. The 'I' is the entity that remains when we strip away all else. The objective world has meaning only because the subjective self exists which is the only thing it could be an 'object' to.

So far, the 1907 edition seems little modified, if at all. Lipps maintains his old objection to treating *Lust* and *Unlust* as properly feelings—they are only colors which feelings take on. The pleasure of the comic is not like the pleasure of friendship, etc. What is unique in each, *i.e.* what is genuine feeling, is rather the element which other psychologists call emotions. The fundamental thing about a feeling is that it is an *Ich-erlebnis*. There are other important things about it, however. Every feeling is *Tätigkeitsgefühl*—a feeling of personal reality, which means at once the reality of *what* we feel, such as anger, fear, or whatever other feeling we 'are.' Another great characteristic of feeling is the inner excitement which we know as the impulse to attain a goal. He employs the word *streben* in a general sense to designate conation, wish, desire, longing. The universal condition of conation (*streben*) is that it is a process having direction and a goal. It is when inhibition arises (*Hemmung*) that conation becomes apparent. He analyzes *Apperceptionstreben* and association and finds in them the above elements of goal-seeking and inhibition.

Lipps distinguishes as passive conation the cases in which an impulse in us, which the whole personality more or less disowns, strives for its goal. Such experiences may appear as a striving for the unpleasant. While enduring it, we feel constrained to seek a certain end which we sanction with reluctance. Opposed to this we have active conation which is the activity of the unopposed personality, which is a striving for the pleasurable, and is the normal condition. Passive conation is really a striving of a fractional part of the self, it implies dissociation, and is not the normal condition. Negativism is to be understood as an instance of passive striving, in which the pleasure-aim of the subject is replaced by its opposite, always poten-

tially present, which now becomes kinetic because of the fact of its dissociation from the bulk of the personality.

From now on Lipps develops and defends the general theme that all experience is fundamentally the realization of selfhood. Whatever end we strive for, we strive to enjoy it. This means, we strive for something which is *realized* in its enjoyment. But, according to his earlier judgment, this enjoyment is an immediate state of the self, hence the attainment of enjoyment is a pure instance of self-realization. For instance, in seeking any sensory experience, we are seeking, not that an object have this sense property, but that we experience this property. To be sure, we may wish to have others to have this experience, but only because this enjoyment by others is something in which we have an interest —and realized interest means self-realization. Association and apperception are processes which realize knowledge for us. But, like a miser who wants gold in a casket so that he may have the feeling of the pleasure of power, so the attainment of knowledge is primarily a pleasure-end. Perhaps these are struggles for phantoms, but the point is that the attainment gives a realization of the self, and this is no phantom. The activity of association and apperception may be play, but they are the play of the soul—it has no more to ask.

The ultimate factor which unites all ends and aims of conation is this fact that they share in one personality, and are its realizations. This is the 'ground' of all conation. However, this is, in the author's case, far from crude hedonism. First, because he emphasizes the higher strivings (as for knowledge) as well as the desires for sense experience. More important still, he denies that spiritual realization is an individual fact. The central aim of all drives is to realize the *human*. We must hold to the principle that "nichts Menschliches kann mir Fremd sein." But this is taught us by reason, to which all strivings must be conformable. He sanctions Kent's law for the will, that we should so 'will' that the humanity within us and in others should have the same goal. This is the 'good will,' which alone is good. But Lipps ends, as he started, by emphasizing the fundamental importance of feeling. "Wir sahen, dass und wiefern dieser 'gute Wille' in seiner absoluten Vollendung das absolut kraftvolle Fühlen und das alles umfassende Denken und Erkennen in sich schliesst."

Wells College C. O. Weber

Les Mécanismes Subconscients. By Georges Dwelshauvers. Paris, Félix Alcan, 1925. Pp. 145.

Dwelshauvers attempts an experimental examination of the conscious and unconscious mental image, in the endeavor to trace its characteristics, functions, and relationship.

His experiments involve (1) the exposure of situations, words, and phrases calculated to arouse typical images; (2) an examination of "l'attitude musculaire," in which the subjects were required to draw a series of lines to the beating of a metronome; (3) a study of the relation between the "reflex graphique" imperceptible movements of the fingers called out by attention to a swinging pendulum—and the mental image; (4) a study of the image resulting from exposure of certain complex geometrical figures; (5) an examination of the performance of various successions of hand and foot movements to demonstrate the relation between the image of a motor act and its resultant execution; and (6) a comparison of ability to draw a series of lines with and without the distraction offered by simple arithmetical problems.

Our subconscious life is, Dwelshauvers believes, a mass (polypier) of images. The mental image is subconscious in its formation and characteristics; and, in the subconscious life peculiar to it, participates to an important degree in the physiological activity of the individual. In this

activity the image becomes an intermediary between the organisms and the conscious life of the organism, making it possible for conscious life to "gain a clear idea of the organism." Our muscular attitudes translate themselves into images; "the motor attitude imposes itself through unconscious channels as a necessity for action, and causes to surge automatically into consciousness certain images, schematic in nature and already conceptualized, which are utilized to maintain and perfect movements." Such images are the "inscription on consciousness of the regulatory mechanism, ... and serve as intermediary between the organism and consciousness." In day-dreaming and in dreaming proper, imagery is especially plentiful, negating entirely ordinary rational synthesis, and bringing into prominence the organic life of the individual. The functioning of the organic life presents itself most easily to consciousness through mental images of a visual nature; next in the hierarchy of images comes the auditory, which is less stable than the visual; then the tacto-muscular activity, which but seldom translates itself into images; and, finally, the gustatory and olfactory images, which readily prove their direct affinity with the "subconscious sensitivity of the digestive functions." The constitution of the automatic functions, then, is the internal work of that animated and multiform process which appears as a mass of images in the moments when it imposes itself upon consciousness.

The intermediation of the image is only quasi-conscious, however, since images are few, at best, during the activity of attention or violition. Thus, "we believe," the author says, "that we have recognized the existence, in the nervous system, of a mechanism of regulation and coördination which is no other . . . . . than the mental image itself, and of which the seat appears to be in the mass of associative paths found in that system." It is particularly important, however, that in such work the subjects learn to distinguish between the mental image and association of ideas, since, if such procedure is not observed, experimental examination of the mental image is impossible.

University of Michigan T. C. SCHNEIRLA

Introduction to the Psychology of Religion. By F. S. HICKMAN. New York, Abingdon Press, 1926. Pp. 558.

The psychology of religion has reached the text-book stage: Charles C. Josey's *Psychology of Religion* and the book now reviewed are both elementary texts for undergraduates.

Hickman has evidently labored long and painstakingly at his difficult task, and has achieved a fair degree of success. But one may foresee that an early successor will greatly improve upon this *Introduction*. For the book is heavy, it neither presents new views nor a clear and easily grasped picture of those parts of the psychology of religion which may be regarded as established. If nothing is discussed thoroughly—something quite excusable in an elementary book—everything is set forth in brief paragraphs: introspection *versus* objective study; structural, functional, and behavioristic psychology; the orginal nature of man; the modification of instincts; subconsciousness; sublimation; etc. The book has five parts: Background of the Problem, Major Factors in Religious Experience, Genesis and Growth of Religious Experience, the Nature and Function of Worship, and Psychological Aspects of Religious Belief. Everyone of these parts falls into many chapters and every chapter is abundantly subdivided. The first chapter (The Genesis of Religious Experience) of Part III, for instance, has two main subheads. Under "Incentives and Reactions" are found the following subdivisions: The sense of need fundamental, Lower levels of need, The tendency to idealize, Idealization in religion, The feeling for values, The Growth of the feeling for values, and Religious revaluation. Under the second subhead (Principal Phases of Religions Ex-

perience) there are the following divisions: Religious control of the will, The feeling phase, Rise of the tender emotion, The faith feeling, The thought phase, and Summary—all this in a chapter of 24 pages!

The outcome of this conscientious effort to provide little dabs of information on an endless number of topics will be, I fear, to leave the student in an unenlightened and confused state: he will not have seen the forest for the trees.

Two excellent features are the addition to each chapter of a list of "Questions for study and discussion" and a selected list of references. I note the omission from those references of my books, *The Belief in God and Immortality* and *The Psychology of Religious Mysticism*, although the problems of these books occupy a large part of Hickman's volume.

It is unfortunate that his understanding of religion is such as to include magic. Thus he continues the confusion for which Durkheim and his school are mainly responsible. One regrets also that although the author has defined religion as a relationship with some power or powers higher than man (55), he seems here and there to pass over to the camp of those sociologists who identify religion with benevolent social mindedness. In a section on "The range of religious habit" (82-83) we are told, for instance that "the habitual attitude of brotherly sympathy toward the unfortunate" is a religious habit. Religion thus becomes whatever is socially good!

Bryn Mawr College JAMES H. LEUBA

Das Problem der Wirklichkeit. By ANTONIN PRANDTL. München, Ernst Reinhardt, 1926. Pp. 128.

Professor Prandtl is not concerned with any absolute reality. The latter is supposed to exist entirely independent of us. But it is evidently impossible, he argues, to think something which at the same time is independent of all thought and rests merely upon itself. Everything which is to be present in our consciousness cannot help bearing the stamp of our nature. Idealists and materialists have both been mislead. Their reality, whether it is an idea or the tangible (*das Greifbare*) is nothing but an assumption for which no sound reasons can be discovered. When a philosophy judges the value and reality of all things by an abstract concept, it can yield nothing but *Widersinn*. For this reason the author hints more than once—beware of the spooks of metaphysics.

The criterion by which the author wishes to determine the objective reality of all things, is uniformity of behavior. Reality has been divided, by philosophers and psychologists alike, into a physical and a psychical world. Insofar as the words physical and psychical are intended to denote different objects, this distinction is not valid. To define these terms more correctly the author avails himself of the labors of Mach, Avenarius and Ziehen. He does not accept the views of these three scholars in their entirety; he wants to be more thorough-going than any of them. They did not overcome completely the dualism expressed in the words physical and psychical. The author denies without any reservations the existence of psychical objects different as from physical objects. There are not two kinds of objects: one physical, the other psychical. Reality is one; it is not part physical and part psychical, but either physical or psychical. It is physical when it is viewed as determined by laws independent of us. It is psychical in view of the fact that in order to be experienced, it is dependent upon brain processes. Since the essential characteristic of the real in the physical world is uniformity of behavior, the problem of reality resolves itself for Professor Prandtl into the question: Can the principle of uniformity be extended to the psychical? The psychical consists of neural processes and mental states. Uniformity of mental states cannot be established; there remains uniformity of neural processes. The author is fully aware that the uniformity of neural processes can not be absolute.

Pure necessary uniformity can exist only between concepts. In order to arrive at an absolute uniformity we must follow the procedure of mathematics and form ideal concepts. But to be true, the ideal concepts must be workable, they must fit in with the physical world.

Although it is impossible to prove the uniformity of mental facts Professor Prandtl can point to the uniformity of neural processes. But what he wishes to prove is not that neural processes have uniformity, and therefore they are real, but that the psychical possesses uniformity, therefore it is real. It is not a question merely of adopting the ideal concept of necessary uniformity for the neural processes. Every mental state, the author assumes, has its specific neural correlate, for if he wishes to deduce the uniformity of mental states from the uniformity of brain processes, a complete correlation between brain events and mind events must be presupposed. It is a well-established fact that certain mental states have neural correlates. But "that there is a one to one correlation between events in a mind and events in the brain and nervous system of the body which it animates. . . . is a perfectly enormous assumption unless there be some general metaphysical ground for it."

University of Iowa B. TAPPER

The Tired Child. By MAX SEHAM and GRETE SEHAM, with a foreword by I. A. ABT. Philadelphia, J. B. Lippincott Co., 1926. Pp. xvi, 342.

Most psychologists will welcome this volume because of the innumerable requests which they receive for the recommendation of books on child health suitable for parents and teachers of little training. It is not fundamentally a psychological work although it is not without the inclusion of much material of psychological interest. It is fundamentally a simple handbook on child hygiene designed for the use of parents especially, but also for the teacher and physician who may not be well trained in the subject matter. The style is clear and there are many appealing stories told of tired children and the causes therefore. Numerous photographs help to maintain interest and also to emphasize the precepts given.

The book is divided into three parts. Part one consists of four chapters which deal with the fundamental principles that underlie work and fatigue. These chapters include a sketch of the normal course of physical development with the usual tables of growth figures. Part two comprises many short chapters on the nature of fatigue, its recognition, its causes, and the stories of some typical cases of tired children. The third and last part is devoted to the prevention of fatigue. There is presented some good advice on the management of the tired child, the rôles of the school and the teacher and the parent, the diet, sleep habits, play, home study and the study of music, and concludes with a general chapter on the mental hygiene of the child.

Most of the material of this book is outside the field of the psychologist. It is prepared by authors who have had much experience with the medical problems they present. As the present reviewer is not a physican he is keenly aware of his inability to examine critically much that is contained herein. In fact he does not care to do so. He prefers to think that it is all scientifically sound because it is so happily presented. It would be a pity if it were otherwise. But as a psychologist he could not avoid noting as he turned the pages, some surprising and some disappointing features. In the presentation of the nature of physical growth, for instance, the authors present the old saltatory notion of growth without mention of the fact that the latest studies seem to indicate the contrary. Baldwin's conclusions on this point are not mentioned although his work is listed in the bibliography. Most of the chapters are followed by bibliographies, some of which are very long, but in them one finds references given indiscriminately in all languages and to anything no matter how obscure its place of

publication. The type of reader for whom the volume is designed could not, in most instances, read these references if he lived where they were obtainable. The scientific reader who attempts to use these bibliographies will soon become exasperated by the many defects in presentation. There are frequent omissions of important items, anything in fact from parts of the author's name to the page of the reference may be missing. One wonders also what principle guided the authors in the selection of titles for these bibliographies. The one on fatigue, for example, omits Öffner's valuable summary entirely, includes Weichardt's dubious work, and omits Lee's studies which so effectively controvert the Weichardt myth. The rôle of the teacher's health is presented without reference to Terman's book on that subject. The effects of the loss of sleep are mentioned with an imperfect reference to the study of Gilbert and Patrick but without mention of the later studies by Robinson. Howell's paper on sleep of 1897 is mentioned but Shepard's elaborate monograph of 1914 is omitted. These are not of course serious defects from the point of view of the purpose for which the book was designed; and for that purpose, the instruction of the layman, it should be of great service.

University of Oregon EDMUND S. CONKLIN

Introduction to the History of Science: Vol. I, From Homer to Omar Khayyam. By GEORGE SARTON. Baltimore, Williams and Wilkins Co., for the Carnegie Institution of Washington, 1927. Pp. xi, 839.

Psychologists will wish to have their attention directed to an unusually important publication in the general history of science. Dr. Sarton, the editor of *Isis*, has now issued the first volume of his encyclopedic "Introduction," which is an introduction to the history of science, not in the sense that it is brief or elementary, but because it introduces persons of thoroughly scholarly interests to the primary and secondary sources for a particular period or individual.

This volume covers the period from the ancient Greeks to the end of the eleventh century. The author omits ancient Egyptian and Chinese science, because so little is positively known about either; but the student, who is accustomed to think of the "dark ages" as a time when there was no science, will be amazed at what he finds in this book. To most scientists the chapter headings of the last five hundred pages will be entirely unknown names of important men representing the successive periods: Diophantos, Iamblichos, Oribasios, Fa-hsien, and so on, to Abu-l-Wafa and al-Biruni. There is so much to say of these twenty centuries that Aristotle is reduced to nine pages and Archimedes to three.

In a way the volume is a "Who's Who" and a bibliography. There are, for example, two condensed pages about Aristotle; the other seven are a scholar's bibliography with the informative comment about sources. The condensation is so great that complete sentences are avoided where possible. The book is not for continuous reading; it is, in the author's phrase, a "tool"—a 'machine tool,' one might say, because of its bulk and complication.

Only the work itself will give an adequate impression of its thoroughness and scholarship, of the care exercised for accuracy, of the extent of the informative evaluation, of the meticulous transliteration of Arabic, Chinese, Greek, Hebrew, Japanese, Persian, Sanskrit, and Syriac. (Every Chinese word is expressed in roman letters with the tones indicated by superscripts and a number added which indicates the exact Chinese character among the odd ten thousand.)

Such tools of scholarly workmanship are rare. We may thank the author profoundly, and also be grateful to the Carnegie Institution which made its publication possible.

E. G. B.

The Case For and Against Psychical Belief. By OLIVER LODGE, A. C. DOYLE, F. B. BOND, L. R. G. CRANDON, MARY AUSTIN, MARGARET DELAND, WILLIAM MCDOUGALL, HANS DRIESCH, W. F. PRINCE, F. C. S. SCHILLER, J. E. COOVER, GARDNER MURPHY, JOSEPH JASTROW, and HARRY HOUDINI. Edited by Carl Murchison. Worcester, Mass., Clark University, 1927. Pp. 365, 10 pl.

The papers brought together in this volume were written for the symposium on psychical belief held at Clark University Nov. 29–Dec. 11, 1926. The book is divided, according to the views of the different authors, into 4 parts. Part I contains the papers of the first six authors named above, who are "convinced of the multiplicity of psychical phenomena." In Part II appear the papers of the next four authors, who 'believe,' but who are "convinced of the rarity of genuine psychical phenomena." Coover's and Murphy's papers compose Part III, which gives the views of those who are presumably open-minded but are "unconvinced as yet" regarding the genuineness of the alleged phenomena. Jastrow's paper and excerpts from Houdini's *A Magician Among the Spirits* are given in Part IV as representative of those who are "antagonistic to the claims that such phenomena occur."

Ten writers who are convinced of the authenticity of psychical phenomena are set over against four who are not! This hardly bears out the statement made in the preface, "we guarantee fair play in the conduct of this symposium." Such a preponderance of affirmative witness is misleading; the uninitiated, for whom the volume is intended, will wrongly assume that this represents the distribution of competent opinion upon the subject; whereas, in point of fact, as Jastrow's questionary returns show, just the opposite is true.

The symposium is biased in another important respect. The question of psychical belief is not disposed of by a discussion pro and con of the spiritistic hypothesis. Of equal importance to the spiritistic, is the hypothesis that explains the alleged phenomena in terms of the medium's hypersensitivity and super-normal ability. Of the 10 proponents of psychical belief who participated in this symposium, 8 are professedly spiritistic, the other 2 are non-committal, but they appear to the reviewer to have spiritistic leanings.

One view of psychical belief has many witnesses, a second has but few, and the third has none! The "rôle of the parliamentarian" was badly cast. The volume is too one-sided to fulfill the hope expressed in the preface that it "should remain for many years the authoritative source for expert opinion concerning the case for and against psychical belief."

K. M. D.

Psychological Principles Applied to Teaching. By W. H. PYLE. Baltimore, Warwick and York, 1924. Pp. vi, 197.

Pyle's book consists of a list of 115 'principles' of psychology, every one of which is followed by suggestions for application to schoolroom problems. Alternate pages of the text proper are left blank for the teacher's notes. The author says "the book is intended to serve as a manual for the daily use of teachers in grade and high schools." He presupposes "previous study of psychology" by the teacher and suggests that "the result of the [first] reading should be a definite organization of the facts of educational psychology."

It is to be regretted that the book itself is so markedly lacking in organization. There is no grouping of related topics nor such as to indicate relative importance of items. The 'principles' range from the statement that causes "of individual differences are (1) hereditary and (2) environmental," to the pedagogical dictum that "the teacher should provide

special incentives to effort." Presumably the author's *Psychology of Learning* is to serve as the basis of organization since page references are given to this book. There is nothing on the title page, however, which indicates that the book was designed especially to accompany the earlier book. The lack of organization of the book being reviewed is very likely to convey the false impression that teaching is a matter of applying isolated principles of psychology.

Some of Pyle's statements seem to the reviewer to be rather seriously misleading. For example: "To learn is to become different. If the difference is not retained we have not learned" (70). What degree of permanence must there be reached in order that there be learning? Again: "If a pupil learns to concentrate all his energies on an experiment while in the biology laboratory he will be better able to concentrate on other work" (160). There is no warning against the inference which the reader is likely to make that the pupil will concentrate better in other work.

Among the applications to schoolroom procedure are a number that should provide good points of departure for a class discussion of the principles involved.

Cornell University PAUL J. KRUSE

Psychology for Child Training. By A D. WEEKS. New York, D. Appleton & Co., 1925. Pp. 312.

It seems indeed unfortunate that the popular misconception of what psychology is should be fostered by naming a book of suggestions to parents on the training of children, "Psychology for Child Training." The statement in the preface that "the entire treatment rests upon stated principles of psychology" is unfortunately not borne out in the text. "Instinctive endowment" is stressed through eleven chapters treating such general and such specific tendencies as curiosity and "the travel interest." That the "hunting instinct" is expressed in children's play is supported by the statement that "at a birthday party a peanut hunt proves the most delightful play."

The author yields to the temptation to treat the structural basis of behavior in such terms as, "the child inherits an intelligence mechanism. The physical structures of intelligence include nerve cells, nerve fibres, the brain, and the organs of sensation" (p. 28). "Of special educational significance is the synapse. This term refers to the fitting together of fibres leading from one cell body and those leading to the adjacent bodies" (p. 29).

A contribution to our knowledge of human behavior is made through the presentation of personal reports, presumably by college students, of their behavior—supposedly illustrative of tendencies to fighting, display, fear, and reasoning. The relation of these reports to the suggestions for child training, however, is in most cases not clear. A good example of the "definite assistance" promised in the preface is the sound advice to parents on the child's use of money (p. 227).

The reviewer takes this opportunity to enter a plea with authors and publishers for more honest titles to books.

Cornell University PAUL J. KRUSE

Der moderne Dandy. By OTTO MANN. Berlin, J. Springer, 1925. Pp. 128.

The 'modern' dandy dealt with by Mann is not the present day 'dandy' but the representative of dandyism as found in England and France during the nineteenth century. Brummell, Wainewright, Wilde, Barbey d'Aurevilly, Baudelaire and others who sought "to find expression" by means of "pen, pencil, and poison," or who considered life as an art which has "its modes of style no less than the arts that seek to express it," should be named here. The author gives an analysis of the time and the society

which produced dandyism, and the persons who represented it, and brings home to us the 'ideal' of the dandy, his 'methods,' his 'system' and the 'consequences' of his life: crime, catholicism, perversion or adjustment to 'normal' life.

The book, written in admirable German, represents the first volume of the *Philosophische Forschungen*, edited by Karl Jaspers, Heidelberg. It may be taken as a contribution to the psychology of 'understanding.' A consideration of its method and its results would lead to an inquiry into the type problem. "In the last analysis, the truth of a type is not something which can be directly determined by the rational methods of science" (p. 81). It is, in the last analysis, 'intuition' which creates the unity of a type by connecting apparently disconnected empirical phenomena. All that science can do is to bring out the different relations which have been discovered intuitively and at the same time avoid any undue exaggeration of any of these relations.

It seems to us that the 'truth' arrived at in such a way is strikingly similar to the 'truth' represented in Hebbel's dramas. At the same time Hebbel was, conceivably, more successful in not exaggerating any of these 'symptoms' of an historical period. The question arises whether cultural psychology which seeks to understand can do more than give us the physiognomy of different historical periods. It may even be a fact that the historian relies chiefly on his 'physiognomical tact' in handling 'facts.' Such a result would seem disastrous; but, if a fact, one should not hesitate to draw the necessary conclusions.

Columbia University HEINRICH KLÜVER

The Psychology of Coaching. By COLEMAN R. GRIFFITH. New York, Charles Scribner's Sons, 1926. Pp. xii, 213.

Professor Griffith's book is a study of coaching methods from the point of view of psychology. The athlete offers himself as an excellent laboratory for the investigation of various physiological and psychological problems. The author suggests that "if psychological theory had begun with studies on athletes instead of with studies on reflective philosophers we might have had a more balanced science." The book aims to aid athletic coaches in sharing their knowledge in an effective manner and to present the dignity and social value of the coaching profession. The coach should endeavor to build up personalities as well as to train men for competition in special skills. The profession of the coach should be regarded as centering upon such aspects as morale and self-control, rather than upon technical accomplishments. The coaching profession, so regarded, assumes genuine human value.

The athletic coach has big responsibilities, since he has much influence upon the characters of those with whom he has to deal. Athletic training should look beyond the stadium to the larger game of life. The coach should be an expert in the diagnosis and handling of all sorts of individual differences. Consideration is given in the book to the planning of the practice sessions, with due regard to incentives and the laws and principles of learning. There is discussion on how to present material effectively and on the planning of the season's campaign. A chapter is devoted to the problem of the 'jinx.' Professor Griffith deals with the 'yellow streak;' here is an important group of phenomena and conditions in the field of emotional response. The problem of will power is also discussed.

The book is well worth the reading by every one interested in the training of young men. It recommends itself not only to the athletic coach but also to those who are dealing with men in the class room. For there should come about a more and more sympathetic understanding between the academic and the athletic interests in our colleges and universities.

University of Vermont PAUL C. SQUIRES

Life and Word: An Essay in Psychology. By R. E. Lloyd, London Longmans, Green and Co. 1924. Pp. xvi, 139.

The author's contribution lies in his point of view, that all animals including man have differential vital qualities and certain fundamental common qualities, *e.g.* all animals breathe, the spine is characteristic of all vertebrates, and verbal-thought is characteristic of man. The author lays a great deal of emphasis upon verbal-thought as being the quality, *par excellence*, which sets man off from all the lower animals. He states that "sensation leads to thought and thought to word, but the step from sensation to thought seems much more important than the step from thought to word. The word or sign is merely the expression or fixation of the thought. How far thought is possible and effective without word, in the animal world, is scarcely knowable." Children, primitive races, and adults with certain diseases or injuries, seem to display thought in their behavior, for which they have no words; the same thought becomes known to normal adults and civilized people in the complete verbal form. The author develops this point of view further as a differential vital quality in organic evolution.

He has extended his biological training into the realm of philosophy and psychology, making as his psychological groundwork, the writings of Plato, Locke, Kant, Bergson, James, etc. None of the findings of modern psychology are mentioned. The essay partakes more of a philosophical than of a psychological treatment.

University of Iowa — Roland C. Travis

Educational Psychology: Its Problems and Methods. By Charles Fox. New York, Harcourt, Brace & Co., 1925. Pp. xiii, 380.

The author states in the preface that the "purpose of the present volume is to consider the contribution made by experimental psychology to the study of educational problems." He begins his book with a chapter in which he discusses the theories of mind, mental heredity, and the inheritance of acquired characters. Launching out from this chapter he carries the reader through a consideration of sensory data observation, imagery, feeling, neural basis of habit, meaning, impulses, imitation, memory, mental discipline, suggestion, psycho-analysis, aesthetic education, mental tests, and mental fatigue.

The book is well written. Fox's diction is clear and direct, and his presentation is interesting. One enjoys reading the book. The reviewer's chief criticism is that Fox runs to theory in his discussion, and gives too little space and attention to fact and practice.

University of Iowa — Lee Edward Travis

The Mastery of Fear. By W. S. Walsh. New York, E. P. Dutton & Co., 1925. Pp. xi, 315.

Walsh discusses in a very popular way the importance that normal and hysterical emotional behavior have for the diagnostic purposes of the physician. He thinks that fear is due to superficial causes or to some particular event in the previous experience of the individual, and that the physician, by discovering the root of the fear, can remove it by introducing habits that break it down.

Fear, according to Walsh, is the antithesis of many desirable aspects of instinctive or emotional behavior. On this basis, his classification of

'fear' is an immediate and all-inclusive one, and chapters are devoted to Fear of Old Age, Fear of Illness, Fear of Heredity, Fear of Maternal Impressions, Fear of Public Opinion, Fear of Ridicule, Unconscious Fear, Causes of Abnormal Fear, and The Fear Habit.

University of Michigan — T. C. SCHNEIRLA

The Einstein Theory Explained and Analyzed. By S. H. GUGGENHEIMER. New York, The Macmillan Company, 1925. Pp. v, 336.

A highly popularized statement of and explanation of the main contentions of the new school of physical relativists. The new doctrines of time and space have significance for the psychologist, but this book does not bring out this significance in any fundamental way.

Wells College — C. O. WEBER

Human Experience. By Viscount Haldane. New York, E. P. Dutton, 1926. Pp. xi, 231.

The chapters discuss basic philosophical problems in an elementary manner suitable to beginning students. There is little in this volume which bears on live psychological problems and nothing that concerns experimental psychology in particular.

Wells College — C. O. WEBER

A Laboratory Manual in the Psychology of Learning. By W. H. PYLE. Baltimore, Warwick & York, 1923. Pp. 161.

In the Preface of this manual the author says it "is published in the hope of furthering and facilitating experiments in educational psychology." Pyle feels that laboratory work and class experiments should be included in every course in educational psychology, and his Manual is adapted to that end. He divides learning into various types, *e.g.* motor, associative, ideational, etc., and he gives, in outline form, an experiment or two in illustration of every type.

Cornell University — H. D. POWERS

The Psychology of Elementary Education. By A. S. EDWARDS. Riverside Textbooks in Education. New York, Houghton, Mifflin Co., 1925. Pp. xiii, 333.

Elementary school teachers and student-teachers will be interested in this book, for the author's presentation is striking. The habit theory of education runs through the book, the greater part of which is devoted to learning and to the teaching process. In addition to numerous selected references and questions for discussion given at the end of every chapter, an excellent list of class experiments is given in the appendix.

Cornell University — H. D. POWERS

NOTES AND DISCUSSIONS

The Plus Sign in Modern Psychology

A recent congress[1] of notable psychologists proved to be instructive and significant. It evidently marked a decided trend in present-day psychology. If we should mention names, they would be recognized at once as the outstanding ones in the world of psychological scholarship in this country and in England, France, and Germany. Thus in this essay we are not dealing with specialists of the second rate or the mediocre order. Many papers were presented either by person or by proxy, and thus many data could be gathered, and a large induction could be drawn from the numerous essays, addresses and discussions.

The writer was present at most of the sessions of the conference, although he was compelled to miss one day's sessions, much to his regret. He desires to give his impressions, and add some reflections on the plus sign that ever stood more or less distinctly before his mind while the erudite discussions were going—a plus sign that grows more distinct as he continues, since the congress, to mull over the great and significant situation. It is best, we think, to mention no names; then no one can feel that the personal element enters as a coloring motive into our observations.

It may be added that we listened with an open mind, with a keen interest, and with an earnest desire to know just what were the views of those eminent savants regarding the nature and causes of mental phenomena.

To begin at once, and to be entirely frank at the start, we missed the note of pure psychology in most of the theses which were presented. If there was a paper read at the conference which announced a clarion and positive belief in the mind as an entity, or that recognized man as a dual being, composed of body and mind, we failed to hear it. A few of the speakers did not use their voices very effectively, and so a few sentences may have been lost; but we do not believe that a distinct note of dualism was struck. There were, it is true, a few voices from the floor which at times seemed to indicate a feeling, or perhaps recognition, of the plus sign and its vital significance; but even those men, when they came forward to present their written theses, did not seem to want to ring out a clear and definite statement regarding the reality of the *psyche* as a thing in itself—*das Ding an sich*, as our German friends would say.

Indeed, at the first session some one had the temerity to rise and interject the question whether one of the German psychologists, whose paper was read by another in his absence, recognized the mind as an entity, interrelated with the body and having some measure of control over it; but, to our surprise, almost dismay, the question was ruled out of order as a "philosophical" question; therefore not relevant in a conference where 'pure science' was to be the sovereign spirit. It was difficult to understand why matters of such common elementary and almost universal psychical experience as the human mind should be taboo as belonging to 'philosophy;' for surely nothing can be plainer than that we have what are known the world over as mental experiences—experiences, too, which certainly cannot be attributed to mere material substances. At all events, it would be passing strange for almost all peoples to believe they have minds if there is nothing in the whole universe but matter. In that case matter would certainly perform a strange exploit in causing the vast majority of people to labor under the delusion that they had minds. What is matter about, anyway, to play such tricks on the majority of the people of the world?

[1] The Wittenberg College Symposium on Feeling and Emotion, Springfield, Ohio, Oct. 19-22, 1927.

Afterward in the convention some of the essayists tried to prove that mental phenomena of the emotional order could be explained on a purely physical basis. Some one spoke of the mind as the "so-called mind," although he may have meant it jocosely. Another referred to the mind as a philosophical speculation, as a metaphysical abstraction; while another thought such words as "perception," "concept," and "notion" might as well be banished from language and thought. But one could not help wondering whether the attempt to explain mental experiences by mere physiological processes was not 'speculation' based on a 'philosophy'—namely, the materialistic philosophy. For when a man proclaims that there is nothing existent but material substance, he has entered the field of highly speculative philosophy. Besides, it must be a venturesome speculation which runs up directly against the almost universal experience of the human race; for no one has ever *experienced* that there is nothing but material substance in the universe; whereas thousands, yes, millions of people—sane people, too, to all intents and purposes—have had experiences of a psychical and spiritual character.

The conception of a mind as an entity was girded at by a couple of essayists as something "metaphysical." We pondered much over that statement. If by metaphysical was meant something *above* the physical order, according to the etymology of the word, then, to be sure, the idea of a mind *is* a metaphysical conception, and rightly so; but if metaphysical means, as is often conceived, something abstruse and difficult to grasp, and more or less vague even to keen minds, then there were loads on loads of metaphysics in this convention. We heard many people, even educated college and university professors, admit that many papers were so abstruse or abstract, or both, as to be almost unintelligible to them. Of course, many technical terms, mostly physiological, were used; and that made some of the papers rather difficult for people who had not recently reviewed their physiologies.

We have said that materialism is a philosophy. It is recognized as one of the three monistic systems of speculation, the other two being Idealism and Pantheism. Materialism says there is only one existent substance—matter. Idealism declares that the only entity is mind, the material universe being an illusion, a mere form of thought. Pantheism merges mind and matter into one substance, as Spinoza assumed but one substrate with the two attributes of extension and thought. The materialist attempts to interpret and explain the universe in terms of matter; and any attempt to do that is a philosophy.

The definition of psychology, until recently, has been, "The science of the mind." Its etymology implies that definition—ψυχή and λόγος. The term ψυχή means the soul or the mind. Now, if there is no such an entity as the mind, the term psychology is a misnomer, and its use is injurious to the cause of science, because it constantly suggests to people's thought something that has no real existence. Some people want to define psychology merely as the scientific treatment of "experience." But experience carries a very different connotation from that of psychology. The latter deals with something concrete—an entity, the mind; the former is an abstract term; for if there is an experience, there must be a something that has that experience; and, judged by all the laws of rational thought, that something must be conscious of its experience, otherwise an experience would be impossible. A stone or a block could not reasonably be said to have an experience. Hence to try to explain psychical experiences in purely physical terms is to leave so large a plus sign as to give the impression that the major principle comes after and not before that sign. At least, the attempted physical explanation, in the present status of the case, is largely speculative, even more so than the assumption of a self-conscious agent known as the mind.

A confusion of thought occurred in one of the learned papers. The author tried to explain the experience of fear on a purely subjective physical basis. He jibed at the usual explanation that the experience of fear arises from the appearance of a fearsome object, because, said the critic, that would be tantamount to saying that "you were afraid because you were afraid." Note the confusion of thought. The terrifying object is an objective reality, whereas the terror aroused is a subjective feeling. How can any one think so mistily as to fuse together an objective reality and a subjective experience? If a cross dog should suddenly appear before you and cause you sore affright, you surely would never afterward, in thinking over the adventure, say that the canine and your scare were one and the same thing. *Bene docet qui bene distinguit.*

Now, to enter into the heart of the subject, the stupendous and all-important plus sign in much of modern psychology is this: On a purely physical basis, how can we account for the dominant fact and universal experience of self-consciousness? That is the primary and basic question. The second is like unto it: Without self-consciousness how can we have an experience? For without awareness we could have no knowledge of anything that occurs, whether it were an external event or an internal experience. Arcturus is a very great star, but it does not know that it is sizable; it does not even know that it exists. But you and I are conscious of our existence and of myriads of other facts besides. Hence there is a difference, *toto coelo*, between a self-conscious being and an insensate material object. A denial of this proposition is contrary to all experience, and hence does not belong to the sphere of scientific psychology, but to that of pure speculation and conjecture. Nothing can be proved before the bar of reason and science that does not come within the field of consciousness. Even the most radical behaviorist must be conscious of his attempt to convince the world of his theory. His very zeal is evidence that he is something more than a material monad.

Let us note a few of our experiences somewhat in detail to see how the inevitable plus sign ever follows hard upon any attempt to explain them on a purely physico-chemical or behavioristic basis. Take the experience of sense perception. If any psychologists go so far as to deny anything like sense perception, or to suggest casting the very term into the discard (as we recently heard one express himself), our reply is that such denial and suggestion are contrary to all human experience, and therefore do not belong to the field of scientific psychology, but have wandered off into the mistland of speculation.

We would first analyze the perception of a physical object—say a tree—through the sense of vision. We say we see a tree out yonder in the college campus; but what is the process of our experience? Well, the waves of the ether of space, set into vibration, carry the colors and form of the tree to the eye; they enter through the iris, are refracted with proper coördination by the crystaline lens and the humors of the eye-ball in such a way as to form an image of the tree upon the retina, which may be described as the moving-picture curtain in the rear part of the eye; thence, in some real, albeit inexplicable, way, the image is borne by the optic nerve back to the cortex of the brain, where it breaks out into clear consciousness, and we say that we see a tree. Strangely enough, instead of perceiving the inverted image on the retina, we see the tree standing, right end up, out in the campus forty feet away.

Now, we can trace the purely physical process quite clearly up to the point where the sensation or stimulus reaches the cortex (although, of course, there are a thousand mysteries along the pathway); but right there, at the end of the physical process, something entirely different occurs which constitutes our dramatic plus sign—the image of the tree breaks out into our consciousness! We are aware of the existence of the tree. How is the passage from the unconscious physical process to the conscious

psychical experience accomplished? What is the bridge that spans the chasm between the unconscious and the conscious? On a purely material basis there is and can be no explanation. The plus sign looms like a phantom before us, and we feel that what lies beyond it is the thing of major importance. Everybody knows that mere material substance, as we know it, has no consciousness and sentiency; yet we are conscious of the presence of the tree out yonder. When we walk out to it, we find it there—a real tree. We may test its reality by feeling its bark and by hearing the soughing of the wind through its foliage. At least three of our senses, therefore, attest its real existence to our awareness.

However, there is still more to follow. By a little attention we are aware of a good deal more than a mere tree. We note that the tree has a symmetrical bole, with comparatively smooth bark; that the branches spring up slantingly from the trunk; that the bark is dark gray; that the foliage, the season being autumn, is quite variegated in color. Now a new psychical experience comes into the field of consciousness: perceiving that the tree is beautiful, we are aware of a feeling of pleasure, and we may exclaim, "How beautiful!" Can mere physical molecular motion or muscular stimuli experience a feeling of esthetic pleasure? Surely there looms here another crucial plus sign.

But still further mental experience may occur in connection with the tree. Why is the foliage so variously tinted. In response to this question, another mental functioning power comes to the fore: we remember that there was a light frost a few nights ago which turned the green of the leaves into browns, reds, pinks, etc. Can mere material substance 'remember?' Must there not be a something after that interrogation-point that has the innate power to recall and to bring past observations and experiences into the area of awareness when the proper stimulus from without reaches it?

But the mind can go much further in its relation to the tree. It may reason and schematize, and bring into play the scientific faculty (from *facio*, to work; therefore a functioning power of the mind, and thus etymologically a good term for psychology). To what order of the natural realm does the tree belong? To the organic world. To what phylum of organic objects? To the vegetable. To what family? The trees. To what species? The maple. Can such a complex psychical process be attributed to mere material substance? Surely not. There must be a rational agent or being or entity (call it what you will), that is able to carry on a process of ratiocination such as has just been depicted.

Suppose we try out another kind of sense stimulus in connection with its accompanying psychic reaction. Sitting at your desk reading or writing, certain sounds divert your attention. They are borne on the air in waves, which strike the tympanum of your ear, pass along through the anvil and hammer, and are caught up by the auditory nerve and carried to the proper brain center; then mysteriously the result of the process breaks out into your mental consciousness, and you say, "I hear the playing of the piano." How will you explain the transition of the physical process into the mental experience? There again is an unbridged gap—unless you posit or assume a real agent, the mind, which has the innate capacity for conscious cognition.

It should be further noted that the sound-waves in connection with the piano gave a very different impression and experience from the light waves in connection with the tree. You do not think of saying, "I *hear* the tree," but, "I *see* the tree;" nor, "I *see* the music," but, "I *hear* the music." Can such psychical differentiations be assigned merely to sense impressions, molecular motion, or conditions and actions of the viscera and glands? Again, in listening to the sounds from the piano you experience much more than mere sound; you are arrested and stirred by the beauty of the music if the instrument is skillfully played; perhaps you are so affected that you cease your work at your desk and give yourself up to the enjoyment you

experience in listening to the sweet sounds. Then you wonder who the artist is, and you rush downstairs to meet him, and to thank him for the rare and uplifting delight he has afforded you. All these experiences follow the plus sign which looms up after the mere physical auditory stimulus has come to you. You never think of saying, "My auditory nerve experienced a delightful feeling by becoming conscious." Somehow, explain it as you will, or leave it unexplained if you prefer, you feel that it was your *soul* that was thrilled by the rare music.

Suppose you hear a beautiful hymn or anthem with ennobling sentiment, then you experience a great deal more than the mere concatenation of sweet sounds; there is the important addendum of spiritual emotion and uplift. Could mere physical action and reaction create a spiritual emotion? In other words, can something be gotten out of nothing? Or, to put it still differently, can an effect belong to an entirely different category from its cause? If there is such an experience as a spiritual emotion—and there surely is in thousands upon thousand of cases—must there not be a spiritual agent or entity capable of having such an experience? If man is all material substance, and nothing more, why in the world and how in the world would material substance ever give rise to ethical and spiritual experiences? These are salient questions? They should be answered.

Percepts, concepts, and emotions exhibit *qualitative* differences. Men have high and low conceptions and feelings; also good and bad. Such differences are certainly hard to explain on a purely physico-chemical basis, or as the result of muscular action and reaction, or of glandular secretions, or of visceral operations. Think of trying to explain a high and holy joy or love as the sequence of the normal action of the viscera! Would not that be dragging a noble science down into the dust? If there are qualitative distinctions in men's motives, feelings and actions, there must be an entity capable of cognizing such differences. The best explanation is the admission of a rational mind or soul.

Of course, we recognize the difficulty of the materialistic monist in psychology. He asks, "What is the mind? What do you mean by such a metaphysical something?" Another question gives the best reply: "What is matter?" You believe that matter is the real thing and the only real thing. Then what is matter? No one can tell. If you say, "Matter is composed of atoms and molecules," we ask, "What are *they*?" If they are made up of electrons, what are *they*? If they are the result of vortex rings or eddies in the universal ether, the fundamental question leaps to the fore: What is the universal ether? No one knows, not the wisest chemist or physicist. Pursuing our interrogations to the *ultima thule*, we do not know what matter is in its fundamental essence any more than we know what mind is. Neither do we know what energy is, or electricity, or life. What we know is only phenomena, not noumena (things in themselves). But we know the phenomena of mind as well as the phenomena of matter. Those of mind are self-consciousness, thought, feeling, volition; those of matter are form, color, hardness, softness, solidity, liquidity, inertia. One class is as patent in human experience as the other. Indeed, our mental experiences seem to be the most intimately and immediately known, because all sense impressions must come into the consciousness in a circuitous route through material media.

The normal human mind has also the direct experience of *volition* in numberless instances. Philosophize about it as one may, such psychical facts come clearly within the range of experience. No experience makes a more palpable impact on the center of human consciousness. We choose between alternatives, we determine ourselves, and we know we do—if we know anything at all. The present writer has exhibited this obvious fact of experience in the classroom as follows:

In a few moments, he says, I am going to use my will thus: by lifting my right arm (now hanging by my side) to a horizontal position, then

closing all my fingers except my index finger, and then crooking my index finger four times. Wait a moment! Ready! Now I *will* to do it! And the act is performed precisely as I predicted it would be performed. If ever I was conscious of anything in my life, I was conscious that, at a certain self-determined moment, I exerted my will, lifted my arm, and performed the rest of the experiment. Yes, the major thing of which I was conscious was the voluntary exertion of my mind. This simple act of volition and all other acts of the kind are inexplicable on a merely physical basis.

It must not be supposed from the foregoing discussion that the dualist in psychology denies, ignores or slights the physiological concomitants in mental experience. Indeed, the consistent dualist must believe in interaction, because he recognizes the reality of both the mind and the body and their marvellous and intimate integration. It is evident to the dualist that the Power that brought man into existence made the mind and the body for each other—made them to be companion-pieces—at least, during this earthly life-time. Therefore, what affects the one part of man affects the other.

A simple analysis of experience proves that the condition of the body affects the mind. If the body is in perfect health, the mental processes proceed in the normal way. On the other hand, ill health or violent bodily pain will greatly impede and disturb the action of the mind. This is a matter of common experience. But the opposite is just as patently true. To illustrate, if you have received good news just before starting to your place of business, you walk erectly and rapidly, your step is elastic and sprightly, and your whole frame and movement proclaim the exhilaration of your mental condition. If, however, you have received sad and depressing news, your body becomes stooped, and your feet drag along heavily—that is, if you do not make an effort to camouflage your feeling of dejection. For this reason, and others that we cannot take time to name, the study of physiology is very interesting for the psychologist, and furnishes an important aid in his studies and their practical application.

Science must gather enough data in the realm of reality to form an hypothesis, and then must try out that hypothesis on other facts, to see whether it will explain them adequately or not. We think we have shown the monistic hypothesis to be inadequate. The supposed causes are not sufficient to account for the grand effects. The primary fact of consciousness ever baffles the attempts of the behaviorist and the monist. So let us broaden our hypothesis to that of dualism and interaction, and try it out in the light of experience.

In the first place, this hypothesis comports with the almost universal experience of the race and with the *consensus gentium*, for nearly all people seem to feel that they have minds or souls as well as bodies. It would be a queer trick of matter, if it is the only subsistence, to cause the vast majority of people to feel and believe that they have souls when they have none. That would certainly create a fundamental suspicion that the testimony of matter is very unreliable.

In the next place, on the assumption that man has a mind, that it is constituted a self-conscious psychical entity, and that it dwells within the body as its fitting habitat—on this postulate, we hold, all man's psychical and somatic experiences can be explained. Let us try it out. We are gifted with a visualizing power as a part of our imagination. We may thus visualize the mind as a self-conscious agent ready for action in connection with the brain. Now, whenever a sense impression is carried back into the cortex of the brain through the corresponding physiological channel, there is the self-conscious mind ready to recognize it. When a physical object is seen— a tree, an animal, a man—the mind apprehends it as belonging to the sphere of vision, *i.e.* as a visible object. When a sound is carried back to the proper center through the auditory apparatus, the mind is there on the alert to cognize it as the ringing of a bell, or the music

of a pipe organ, or the singing of a prima donna. The same is true of all other psychical experiences, whether they come through the senses or through rational or abstract mental processes. Every mental fact is known by the impact made by it upon the focus of the conscious self, and only in that way. This self, ever alive and alert in waking hours, may be likened to the operator in a central telephone station, who, being always on the *qui vive*, responds to every signal on the board before him, and calls, "Number?"

Should the objection be raised that we have *assumed* that the mind is so constituted as to be self-conscious and to be able to respond to various stimuli in a self-conscious way, we reply that the physicist has to assume that the ether of space is so constituted as to carry light and color on its infinitesimal undulations; the optic nerve is so constituted as to carry the image of an external object back to the cortex; the auditory apparatus is so constituted as to convey the waves of sound to the proper brain center; the nerves of taste carry back to the appropriate organ of the brain the various sensations produced by the passing of food and drink over the palate.

So reasoning from analogy and testing all data in the alembic of experience, we must conclude that the mind is so constituted by its Creator as to be conscious of itself, of perceptions, reasoning processes, moral feelings, spiritual exaltation or depression, and all the other varied facts and phenomena that come within the arcanum of its awareness.

Wittenberg College LEANDER S. KEYSER

CERTAIN IMPLICATIONS IN THE CONCEPT OF *GESTALT*

It is well-nigh impossible to present a new idea without arousing a great variety of misunderstandings, for the clothing of a new idea in words is like the putting of new wine into old bottles.[1] Nobody can be blamed for seeing the bottle before its contents. There are, however, certain intrinsic sources of error, two of which are mentioned here, that bear upon most scientific discussions.

First, the originator of an idea is forced to use concrete illustrations and he of course chooses them from fields in which it is easiest to establish his points, *i.e.* from fields in which the greatest advance has been made and in which the chance of meeting unanswerable questions is reduced to a minimum. This procedure—especially if the entire proof of the hypothesis rests upon it— is, however, likely to lead others to the conclusion that the field from which the illustrations are drawn is somehow essentially related to the idea that is being advanced. For example, some articles by Köhler and Koffka, which were concerned with facts of shape more than any other of the early studies on perception,[2] gave some people the mistaken impression that they considered shape to be of greater importance than qualitative and quantitative properties of the visual perception.[3] Since the *Gestalt* theory considers shape, quality, intensity, and quantity as interdependent and does not apply more specifically to one than to any other, arguments against the primacy of shape (of any one of them) does not affect the central idea of the *Gestalt* theory.[4]

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[1]The writer wishes to acknowledge his indebtedness to Professor Max Wertheimer, Professor Wolfgang Köhler, and Dr. Kurt Lewin of the University of Berlin, who have, during the last five years, introduced him to the theory of *Gestalt*. He is also greatly indebted to Miss I. G. Probst, Miss A. Mathiesen, and Professor C. A. Ruckmick for their assistance in the preparation of this note.

[2]W. Köhler, An aspect of *Gestalt* psychology, *Ped. Sem.*, 32, 1925, 691-723; K. Koffka, Perception: An introduction to the *Gestalt-Theorie*, *Psychol. Bull.*, 19, 1922, 531-585.

[3]Harry Helson, The psychology of *Gestalt*, this JOURNAL, 37, 1926, 205, 213 ff.

[4]Köhler points out (*op. cit.*, 692-702) how the perception of things and consequently the perception of shape depends on the qualitative and intensive properties of the visual field.

In the second place, every new hypothesis has to be defended against other existent hypotheses. Thus in Germany *Gestalt* psychologists argued against Wundt's hypothesis of 'creative synthesis' and also against Meinong's and Benussi's 'production theory'—neither of which does justice to the importance of the objective configurational aspects of the stimuli. *Gestalt* psychologists, therefore, emphasized the importance of the objective facts. Now a reader who does not believe in the 'production theory' is easily lead to the opinion that the doctrine of the *Gestalt* implies that subjective determinants are of no importance in perception.[5] Any argument against such a position does not touch in the slightest degree the central idea of the *Gestalt* theory. Similarly, *Gestalt* psychologists argued against the negation of consciousness by behaviorists, with the unexpected result that at least one non-behavioristic psychologist was led to believe that the *Gestalt* psychologists pretended to be the only, or at least the chief, investigators into consciousness.[6]

The list of misunderstandings could be greatly extended. The only purpose of mentioning them here is to explain why for the moment—and fifteen years are only a moment in the development of science—certain misunderstandings are inevitable. I mention the two above simply because the sources of error appear very clearly in them.[7] The only outcome of such misinterpretations is to learn that the original exposition was ambiguous and that under certain unexpected conditions was even positively misleading, and consequently to try another approach to the subject from a new angle. An approach, which has not as yet been tried, consists in taking some of the actual cases in which the term *Gestalt* is used—chiefly cases where it is not used in accordance with its original meaning—and in explaining how the real concept of *Gestalt* would have applied there. An example of this method is offered in the following paragraphs.

Mogensen and English had an original and happy idea of investigating the apparent warmth of colors.[8] If the warmth of colors has something to do with real warmth, they argued, it must have some influence upon the tactual perception of warmth. They exposed pairs of electrical resistances at equal temperature and covered with various brightly colored papers. The observers, without knowing that the resistances were equally warm, touched them while looking at them and then gave a judgment as to which one of a pair felt the warmer. The number of times that every one of the colors used was judged the 'warmer' was as follows: green, 416 times; blue, 405 times; orange, 380 times; yellow, 377 times; red, 371 times; and purple, 301 times. This was an unexpected result. The experimenters, and I, too, had expected exactly the opposite outcome, since green and blue have the least apparent warmth, and red and purple the highest. The experimenters were disappointed. "Although the data are slight," they say, "the conclusion seems warranted that the apparent warmth of colors is *insufficiently intrinsic to enter into a total configuration or Gestalt* in such a way as to modify the judgments of tactual warmth."[9]

Must we really give up so soon? Let us see. We are not told explicitly what kind of influence of color-warmth on tactual-warmth the experimenters expected as an effect of the formation of a *Gestalt*. We can, nevertheless, discover what they expected from what—to their disappointment—did not happen. It did not happen that the red or purple objects that *look* warmer *felt* warmer, that the blue or green objects that *look* colder *felt*

[5] Cf. Helson, *op. cit.*, 205 ff., esp. 213; also H. R. de Silva, An experimental investigation of the determinants of apparent visual movement, this JOURNAL, 37, 1926, 469-501; esp. the summary.

[6] Madison Bentley, The major categories of psychology, *Psychol. Rev.*, 33, 1926, 87.

[7] That I take an American instance does not imply that equally flagrant German examples could not have been selected.

[8] M. F. Mogensen and H. B. English, The apparent warmth of colors, this JOURNAL, 37, 1926, 427 f.

[9] *Op. cit.*, 428. Italics are mine.

colder. It must, therefore, be supposed that they expected visual warmth to enhance tactual warmth, and visual coldness to diminish it.[10] To be more explicit: it was expected that a certain amount of visual warmth (either a positive or a negative warmth) would be added to the given amount of tactual warmth, viz., that the apparent warmth underlying the judgment of the observers would be the total of the two kinds of warmth in question. In other words it was supposed that the attributes of a whole could be found by adding the attributes of its parts.

Is that to be expected of a *Gestalt*? Not at all! Such an expectation is a direct contradiction of Von Ehrenfels' negative definition of *Gestalt*. According to Von Ehrenfels no prediction can be made from a consideration of the attributes of the separate parts regarding the attributes of the whole. In color-contrast, for example, we have the paradoxcial fact that when red is added to red, *i.e.* a red spot is put on a red background, we get not more but less red than before. In this way color-contrast shows most convincingly how *'gestalted'* units behave. Such facts, in the meaning of *Gestalt* theory, are not therefore exceptions for which specially adapted nervous apparatus must be assumed, but rather illustrations of the true nature of psychic processes.

But if *Gestalten* are not the total of the part attributes then what are we to expect? Provisionally it can only be said that, as a symptom of having entered a *Gestalt*, the total of the part-attributes most probably will not be found, for the attributes of the former parts will be altered in some manner. In order to get a positive answer to this question, we have first to see where, if not in the attributes of the parts, we should seek the new attributes of the compound. This is shown by Wertheimer's adequate interpretation of the positive definition in 1924,[11] according to which *Gestalten* are units of such a nature that what occurs in their parts is controlled by intrinsic laws of the structure or character of the actual whole. The meaning of these laws of *Gestalt* obviously implies that only a very small number of such laws can be found which are valid in, and can be applied to, all the various kinds of wholes, for what laws are effective depends wholly upon the special character of the particular whole that is actually given. In perceptual formations it generally depends, for the greater part, on the actual configuration of the external stimuli, from which it arises.[12] According to Wertheimer's definition, as is obvious, this occasioning physical configuration itself is not necessarily a *Gestalt*, as will be made clear in our further discussion.

Our first task, therefore, in the search for the attributes of the compound, is to become thoroughly acquainted with the special physical configuration in question, *i.e.* to analyze it exactly and wholly. But we are told that analysis is strictly prohibited in *Gestalt* psychology, that only unanalyzed wholes are allowed to be considered. We must admit that we never could understand how, in this way, any scientific results could be obtained. In fact analysis is prohibited; or rather, as *Gestalt* psychologists believe (and in many cases have found to be true), analysis does not help to discover the nature of a compound. But this concerns only a certain rather widespread kind of analysis, such as "separating a given whole into its elements, viz., into its constituent parts and considering these parts separately," as in the case which we were considering, separating the perceived warmth into its tactual element and its visual element and hoping to get the apparent warmth by putting these elements together. This kind of analysis led to no result. But there is another kind of analysis which we are not only *allowed* to apply, but which is wholly indispensable and, in the opinion of *Gestalt* psychologists, has never been carried far enough in older psychology. This is the minute and full description of all

[10] As I prefer to call it, for both tactual and visual warmth are *apparent* warmths.

[11] Max Wertheimer, *Über Gestaltheorie*, 1925.

[12] Exceptions to this rule will be discussed later.

details of the fact in question, of all the parts, part-processes, and part-qualities of the whole, just as they look or behave at their particular place in the actual whole, and would not look or behave elsewhere, neither at another spot of the same whole nor as a part of a different whole nor as separate independent things.[13] It is not necessary to consider here the wider circumstances under which the facts observed happened, or to mention what external and internal 'factors' may have modified the result, as *e.g.* bad sticky air, or fatigue, or a headache, or an insufficiently concentrated attention of the observer. These also are important facts, but in most cases they are not so important by far as the structure or configuration, in space and time, of the specific external stimuli themselves.

In our example this structure was as follows. There was a warm object the surface of which was homogeneously covered, as long as the object was only looked at. But at the moment when it was touched its surface was partially covered—that is to say, optically interrupted—by the hand of the observer. At the spot covered by the hand the color and, in consequence, the *visual* warmth of the surface was not perceived, but only tactual warmth; on the other hand, this area in which tactual warmth was perceived was rather distinctly limited by the size of the hand and somehow, at least in two directions, *surrounded* by the still perceivable visual warmth of the uninterruptedly visible remainder of the surface. (If the authors had applied electrical resistances exactly the size of a hand, and thus wholly covered, an analogous circumstance would have resulted in *time*, as in their experiment it resulted in *space*.) These are, so far as I can see, the essential traits of the configuration of stimuli *in each member* of the exposed pairs, the configuration being independent of the colors applied. Perhaps we can find out now what kind of *Gestalt* laws are valid in such a configuration, what modifications the part-qualities will undergo, in how far *the apparent Gestalt will deviate from a mere configuration of sensations* which would exactly correspond to the configuration of physical stimuli.[14]

Since only a relatively small number of special *Gestalt* laws have at this time been definitively formulated, we are not able in every case to predict the laws actually valid from the description of the configuration. For we may possibly encounter a type of configuration not yet explored; in this case our task would be to fathom the function which is valid as between the configuration in question and the results found. This would be a contribution to the discovery of new laws. But in the actual case we are not in such a difficult situation. Generally speaking, we have two kinds of like sensorial quality spread over a surface and clearly separated, so that the less extended of the two is, at least partially, enclosed by the other. These are just the conditions under which in all known cases, *contrast* will occur at its greatest intensity. If we consider once more the results of Mogensen and English, we are not so disappointed as they are, since the rough outcome of their experiment, which the authors themselves are inclined to ascribe to mere chance and therefore to neglect, is just what was to be expected from the configuration in which the stimuli were exposed.

No doubt, the result is very slight and there are a great many insignificant and even contrary cases. But here our task is primarily to get a

[13] For such 'analyses' see, *e.g.* K. Koffka on Perception, etc., *Psychol. Bull.*, 19, 1922, 540, 553 ff.; W. Köhler, An aspect of *Gestalt* psychology, *Ped. Sem.*, 32, 1925, 696 ff.

[14] A word on the meaning of the terms 'Configuration' and 'Gestalt' in German psychology. In our example, visual and tactual warmth of the papers would have been in *configuration* in any case (in such a relative situation and proportion of extent in space or time as can be stated in geometrical terms, such as being close together, one surrounded by the other, and the like). But visual and tactual warmth are in a *Gestalt* only if a greater or lesser mutual modification is possible. This mutual modification that results in a deviation from the 'total' is just the decisive criterion between mere configuration and the union into a real psychical *Gestalt* —as Mogensen and English did correctly suppose but did not adequately apply to their experimental conditions.

finer and more extended configurational analysis. We have to consider the relative size of the covered and the visible parts of the paper. We have to consider what colors were compared in each pair and how, perhaps, the positive, negative and insignificant results are correlated with certain kinds of color-configurations within the pairs. We have to consider the consequence of exposure, for it is possible that certain preceding influenced certain following colors in a direction opposite to contrast. We have to consider that the psychical state of the observer may enter the configuration, that opposite results may originate from different inner attitudes which, as is known, are the more effective the less unambiguous the configuration of the stimuli. Perhaps our conditions are not unambiguous enough for clear results, *e.g.* when two colors are very similar to each other there is no unambiguous color-contrast but a fluctuation between contrast and assimilation. The same thing may be possible in our experiment. That is to say, we have to consider the relative rates of the visual and tactual warmths used. Finally, we have to consider the relative sensibility of our various observers for color warmth, which naturally will modify the results. Possibly we have among them a few with a very high threshold or even total insensitive to warmth.

Starting from our assumptions, and with a refined and extended analysis, our second task is to improve the conditions, to modify them either in such a way that contrast will certainly be strengthened or, on the other hand, just as certainly will be converted into its opposite, the assimilation or mixture. In order to strengthen contrast, according to well-known rules, we have to make the covered spot smaller and more neutral in warmth, the surrounding surface larger and, if possible more brightly colored. In order to obtain assimilation or mixture (the result expected by our authors), we have to avoid interruption of a larger surface of the one kind by a smaller of the other kind; we have even wholly to avoid the possibility that the two kinds of stimulation are so situated that one is beside the other with a common boundary. Perhaps it will be suitable to create a kind of 'atmosphere' in which both kinds of warmth simultaneously contribute their share, *e.g.* to let the observers enter a number of small rooms which have the same temperature but are warmer or colder by the color of their walls or of their illumination. Perhaps it will even be possible to adjust the temperature of the rooms so that they will have the same apparent warmth, in spite of different temperatures.

Only by such an improvement of the experimental conditions will it be possible to prove whether the visual and tactual 'warmths' are merely configured or enter into a real *Gestalt*.

SUMMARY

I have attempted to show that an inadequate use of the concept of *Gestalt* has led to false expectations concerning the outcome of experiment; that an adequate use of the concept would have promoted a contrary expectation; that such a contrary result was obtained but was neglected by the experimenters in consequence of the false expectations; and, finally, that certain modifications of the experiments are suggested by the concept in order to obtain clearer and more effective results.

University of Iowa WOLFGANG METZGER

EMPIRICAL PSYCHOLOGY

Carmichael[1] and Boring[2] have recently pointed out the implications of the term *empirical psychology*. Carmichael has differentiated three meanings and urges the use of the term in the sense of experimental. Boring objects to this synonymous usage of the two terms and prefers to sub-

[1] L. Carmichael, What is empirical psychology? this JOURNAL, 37, 1926, 521-527.
[2] E. G. Boring, Empirical psychology, this JOURNAL, 38, 1927, 475-477.

stitute 'empirical' for the more awkward term 'experiential.' It follows that experience and experiment for him are not equivalent terms. With this general position I should agree although I can not literally admit his statement that empirical psychology is necessarily "something broader than 'experimental psychology,' something which includes experimental psychology, for to my mind certain types of empirical psychology would not necessarily be experimental. Thus much that has been written by Freud and his followers might very well be empirical psychology but for the most part not experimental; and certainly much of the intuitive psychology of the present day as we find it in Roffenstein,[3] Jaspers,[4] Pfänder,[5] and Scheler[6] belongs to the same category. In my classificatory scheme I have preferred to take the historical position,[7] opposing rational and empirical as coördinate and subdividing empirical psychology into theoretical and experimental.[8] Of course, as Carmichael has pointed out, this may not be strictly speaking the historical point of view since Wolff made the basis of his distinction between rational and empirical the essential difference between the mental faculties involved in the discussion of these divisions. When their underlying scientific principles were derived from rational faculties they were known as rational; when they came from experience they were designated as empirical.[9]

While classifications indicate simply shifts of emphases where the stress in one case may be more in the direction of one category and less in another, I think in the main the various psychological approaches can still be put under their classificatory rubrics. A purely rational type of psychology worked out by the *a priori* method of reasoning in which concepts were analyzed into their fundamental meanings has very much disappeared. This was largely the psychology underlying the writings of Descartes-Leibnitz, and Kant. With a shift towards the actual analysis of experience, whether on theoretical grounds or mainly on the basis of theoretical consciousness or whether on grounds of strict experimental procedure, empirical psychology has developed and survives today. Empirical psychology in other words, to my mind, means any form of psychology whether theoretical or experimental, or both, which collects its data from experience, and which draws its conclusions only after these data have been collected and examined for points of similarity and difference *in their own*, not in their *conceptual*, nature.[10]

In experimental psychology the procedure may involve theoretical approaches in general, as when experiments are performed by certain

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[3] G. Roffenstein, *Problem des psychologischen Verstehens*, 1926.
[4] K. Jaspers, *Die Psychologie der Weltanschaungen*, 1920.
[5] A. Pfänder, *Einführung in die Psychologie*, 2nd. ed., 1920.
[6] M. F. Scheler, *Die Transzendentale und Psychologische Methode*, 2nd. ed., 1922.
[7] C. A. Ruckmick, A schematic classification of general psychology, *Psychol. Rev.*, 33, 1926, 398.
[8] F. Thilly, *The History of Philosophy*, 1914, p. 380.
[9] Klemm states that Wolff's rational psychology was built around the power of representation as a fundamental power of the mind whereas the empirical aspect of psychology depended upon the faculty of sense-cognition. O. Klemm, *The History of Psychology*, 1914, 59.
Cf. also W. Wundt, *Grundzüge der physiologischen Psychologie*, 6th ed., I, 1908, 6 ff.; *Grundriss der Psychologie*, 8th ed., 1901, 20 ff. Wundt shows very clearly why it is impossible to separate in practice *rational* or *metaphysical* psychology from empirical. Even the most orthodox "arm-chair" metaphysical psychology will be concerned primarily with "experience!" But the procedure is for the most part different, and, of course, the objective is distinctly different. He gives a schema in the *Grundriss* which distinguishes, under rubrics of the first order, *metaphysical* and *empirical* psychology and then, under *empirical* psychology, the psychology of the "inner sense" or pure self-observation, and psychology as the science of immediate experience of which there are two branches, the psychology of peoples and experimental psychology.
[10] In this historical classification it is a puzzle where to put so-called extreme behavioristic forms of psychology. Physics can be called empirical when it obtains its testimony in the last analysis through the senses, *i.e.* as when voltmeters are read and electron deposits are noted through vision. But behaviorism in this form, of course, at once uses experience to gather in the testimony but denies it as material for science. Experience in this sense is not, therefore, the fundamental datum for this form of psychology!

schools of psychological thought, or it may develop out of a tentative hypothesis. In these respects there is, of course, involved a certain amount of deductive reasoning. But from this point on the procedure is characteristically scientific in that logical thinking of this major deductive or speculative sort is suspended until sufficient data have been assembled and the hypothesis is reëxamined in the light of the results of experimentation. The history of psychology has shown that in many cases opinions have been reversed, if not radically at first but certainly in time, as the controlled results from laboratory experimentation have either failed to corroborate the original thesis or have accumulated increasing amounts of data on the opposite side. Anyone who has studied carefully the intricate process of thinking knows, of course, again the fallacy of the arbitrary separation involved in the conceptions of inductive and deductive logic. Pillsbury has admirably shown how impossible it is to follow exclusively one or the other procedure. We are obliged to characterize by the average or predominant process knowing how artificial, in general, classification is.

Now there are so-called psychologists, of course, who elaborate upon a theory of consciousness, of mind-body relationship, or of emotion, who do not resort to experimental methods to substantiate their hypotheses. Even an experimental psychologist may write a theoretical treatise that could be classified under the category of theoretical psychology without quoting experimental data to substantiate his theory. Experimental psychologists furthermore at some time or other must resort to theories or explanations of data obtained in the laboratory. So, also, when any theoretical question arises the proof or disproof of it must be based ultimately for experimental psychology on actual investigation in the laboratory.

I think, therefore, that there is still considerable utility for a clear-cut distinction between the terms empirical and experimental and I should be the last one to agree with Carmichael's conclusion, although I think his analysis of the historical use of the term is admirable in many respects. I suppose the reason for the attempt to make the terms synonymous lies in the fact that today there is less call for a distinction between rational and empirical psychology and, turning the logic about, that most scientific psychology today since it is experimental is also empirical. But to any student of general movements especially in Germany in the present day, one must not overlook the fact that much systematic psychology is empirical in the sense that it is based upon actual human experience even to the point of its phenomenological origin, but that it does not always take pains to corroborate its own position with experiments done in the laboratory. Whether we assimilate *Gestalt* psychology or not we must admit that this new movement, for one, has not only proceeded to prove its claims through experimentation, but has provoked experimental procedure in new directions and has thus stimulated the experimental method throughout the whole field of psychology.

University of Iowa CHRISTIAN A. RUCKMICK

A DEMONSTRATION METHOD OF STUDYING THE LEARNING PROCESS

In order to demonstrate to classes in elementary educational psychology, some of the many objective and subjective phases of learning, I have developed a series of 450 problems involving multiplication of two-place numbers by two-place numbers. In this series of problems all zeros and ones were omitted, and all problems whose answers involved fewer than four digits or which were missed more frequently than the usual number of times, were removed from the series. The remaining problems are approximately equal in difficulty since the records of performance of students show the same relative results when the students begin with any series

or with any problem in a series. A tabulation of the problems missed shows that no problem was missed much more frequently than another when the factor of position near the end of the daily task was accounted for. The series of problems was divided into 15 tasks of 30 problems each. A series can be given each morning during the first 6 min. of the class period without disrupting the work of the period to any considerable extent.

The students may work uninterruptedly for the entire 6 min., or the instructor may call out the minutes as they pass and require the students at the end of every minute to draw a line under the last completed answer. This series of problems has proved very satisfactory in demonstrating many of the phases of learning to the students. The introspective observations of the students on their own processes and methods of work prove to be of as great interest to them as do the scores which they make from day to day. The problems were mimeographed on suitable paper, two columns of 30 problems each to a page, double spaced, and with margins on either side of the columns. These pages were then cut and trimmed into sheets $4\frac{1}{2} \times 11$ in., each bearing a daily assignment.

The weaknesses of the procedure were (1) the tendency of students to practice on similar problems outside of the learning period in spite of requests that they refrain from this; and (2) the devising of a satisfactory scheme for equitable scoring in terms of effort and accuracy. In using this series of problems I returned the scored problems of the preceding day to the students in order that they might inform themselves of their scores, their types of errors, and the correlations between their introspections and the results of the work done in connection with them. These sheets, however, were laid aside when the multiplication for the day was begun.

When I first began to use this series of problems, I considered scoring only the entire answer to every problem as right or wrong. This made an index that was too coarse to show well the gains and losses in accomplishment. As a result I scored every digit of every answer—the score for the day being the number of correct digits in the answers obtained. It is obvious, however, that the processes involved in finding the different digits are of very different degrees of difficulty. I am therefore now using a system of weights to equalize the errors that occur within the different digits. I indicate an error, beside the answer in which the error occurs, in the thousand column by 'a,' in the hundreds column by 'b,' in the tens column by 'c,' and in the units column by 'd.' These symbols can be counted quickly and the number of each multiplied by a constant that was empirically determined.

These constants were found by tabulating 6,866 errors according to their places in each of the four columns (units, tens, etc.). These numbers were then reduced to percentages of the entire number of errors, and the reciprocal of each was found and designated as the constant for the errors in that column of the answers. The number of errors by columns was: thousands, 1171; hundreds, 3386; tens, 2125; units, 184. The constant or weight that was used for the errors in the column of thousands was -0.8; of hundreds, -0.5; of tens, -0.7; and of units, -1.0.

The amount of work involved in scoring a number of papers by this method is slightly greater than that involved in the simpler methods of scoring each incorrect digit in the answers as -1, but it gives scores that seem to be more nearly in accord with the amount of effort and attempted accuracy involved. The virtue of the method lies in the discussions that arise from comparisons of scores by the students and the discussions of working methods when they receive scores from the two methods of scoring mentioned above.

Whitman College H. R. Laslett

The Wittenberg Symposium on Feeling and Emotion

A symposium on feeling and emotion was held at Wittenberg College of Springfield, Ohio, from October 19–22, in connection with the dedication of a new psychological laboratory. The papers read at this meeting undoubtedly represented the greatest assemblage of psychological authority ever brought together in this country, for there were papers by most of the leading psychologists of both Europe and America.

Since the papers, 15 European and 18 American, are soon to be published in book form it is needless to attempt a detailed summary of every one. Moreover, there seemed to be a fairly general belief that we shall have to await the critical study of the papers to gauge at all accurately their true import. It was a very heavy program presented to packed houses. Anything like a comparative evaluation was rendered still more difficult by the fact that the papers on emotion, especially, were the logical development of *a priori* assumptions and empirical facts. Until the basic assumptions are brought into harmony or recognized as irreconcilable any formulation of a general nature will be futile.

It is always in order, apparently, to bewail the lack of genuine experimental work in this field. Here the physiologists hold a distinct advantage. One wonders, to cite only a few cases, whether any type of report would reveal the "objects, occurrences, and relations" of Dunlap, the teleological "tendencies" of McDougall, or the "energies" of Morton Prince. The fact is that here as elsewhere there is a wide divergence between postulates with no evident principle which will bring them together. Even an external teleological factor, such as Carr proposes, will scarcely add to our understanding of the emotions as such.

This leads to the functional consideration of the emotions as presented by Pillsbury, McDougall, Prince, Dunlap, Claparède, and Howard. This does not imply that the treatment in other papers was not functional in intent. But who can say whether the emotions help or hinder in the economy of life; each view had its advocates. Such disagreement does not augur well for the direction and control of the emotions; it again savors strongly of the reflection of *a priori* principles.

There was one fairly well marked trend that should prove wholesome. Several papers voiced the opinion that the feelings and emotions must not be considered as special discrete moments, *i.e.* specific entities, in our lives but rather as a current in the total stream, which at times, to be sure, makes up the total but which more often runs along with other processes. Viewed from this angle mind in its functional character does not present clear cut cleavages; it is rather a matter of organization, of focus and background, of wholes and part wholes. We find that experience at every moment presents a total or pattern which, often at least, betrays a greater or less resemblance to the affective or emotional aspects. If these aspects become too strong, as Washburn found, the thought processes lapse.

Hence in the future we may expect to find greater emphasis placed upon mind as a unity of experience, externally and internally aroused, with varying degrees of different aspects differently patterned. This appears hopeful. But as stated above, the real worth of the synposium can be realized only through a critical study of the papers in published form.

Cornell University L. B. Hoisington

The Washburn *Festschrift*

The thirty-ninth volume of the Journal, published as a single number in December, 1927, was dedicated to Margaret Floy Washburn in commemoration of her completion of 33 years of distinguished service to psychology, including 25 years as professor of psychology at Vassar College, and as a member of the editorial board of this Journal.

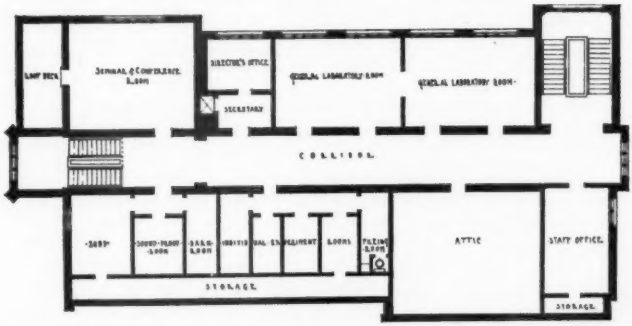
The Volume contains 32 studies in various departments of psychology, which were contributed by Professor Washburn's colleagues and former students.

Orders for the Washburn Commemorative Volume should be sent to D. R. Knight, 43 Morrill Hall, Cornell University, Ithaca, N. Y.

K. M. D.

THE NEW PSYCHOLOGICAL LABORATORY OF WITTENBERG COLLEGE

A new psychological laboratory has just been completed at Wittenberg College, Springfield, Ohio. It is located on the third floor of a new building that is shared by the departments of chemistry and psychology. It consists, as is shown in the accompanying floor plan, of a specially constructed sound-proof room, of a dark-room, a shop for the lighter work—a modernly



equipped machine shop is located in the basement and is shared with the department of chemistry—of four small rooms for individual research, of two large rooms designed especially for introductory classes in experimental psychology, of offices for the director and for the staff, and of filing rooms for departmental records. All the rooms are modernly equipped and all the floors are covered with heavy battleship linoleum.

Of the rooms on the lower floors of the building, a large auditorium with an accompanying demonstrational laboratory, several class rooms, and a room for a departmental library have been allotted psychology.

Wittenberg College MARTIN L. REYMERT

EDITORIAL DEPARTMENTS

With this number of the JOURNAL a new department, Notes and Discussions, is begun—an expansion of the old department that appeared under the title Notes. In this department the JOURNAL will print all discussions of articles, replies to them, criticism of them, or controversy about them. It will also print here other long informatory notes and discussions, and all material of interest to psychologists, and without regard to length, that does not properly belong elsewhere. Naturally the short communications will follow the longer ones.

Recently the JOURNAL began a separate department under the title Apparatus. The JOURNAL welcomes for this department both long descriptions and short notes that describe new apparatus or improvements upon old apparatus, when there is reason to believe that new construction is of general scientific interest.

The JOURNAL will thus have five separate departments: original Articles; Minor Communications of the nature of the Minor Studies which have long been familiar to its readers; descriptions of Apparatus; Book Reviews; and Notes and Discussions. Original articles will be printed in large type (10-point) or in mixed type sizes (10- and 8-point), according to appropriateness and length. The other four departments will be printed entirely in the smaller type (8-point). The JOURNAL solicits for this policy the coöperation of its contributors, who, although sometimes preferring the larger type for their communications, will readily realize that there is at the present time great pressure for space upon all the psychological journals, and that these rules enable this JOURNAL to serve the interests of psychology very much more extensively than would otherwise be the case.

E. G. B.

THE DATING AND PUBLICATION OF MANUSCRIPTS

Beginning with this number of the JOURNAL, the date of the acceptance of an article for publication will be given as the first footnote. Manuscripts will be dated as they are received, and divided into two classes: long articles, extending from 25–50 pages, and short articles. Within each class publication will follow in the order of receipt, in so far as such procedure is consistent with a balanced number. In every number of the JOURNAL there will appear 2 or 3 long articles and from 5–10 short articles.

Articles accepted for publication in the Apparatus and the Notes and Discussions departments, since they are usually short and timely, will be run at the earliest possible date.

K. M. D.

ERRATUM

While working upon apparent movement in our laboratory, one of my students discovered a discrepancy between his results and my statement of Cermak's fourth law in this JOURNAL, 36, 1925, 512: "the greater the time of exposure, just so much the greater must the pause be between the phases if any kind of movement or any degree of fusion is to be seen." Referring back to the original (*Psychol. Forsch.*, 1, 1921, 106), we find it plainly stated that "je grösser die Expositionszeit, um so kürzer muss die Pause sein" which is in agreement with our laboratory findings and shows that the fourth law should read: "the greater the time of exposure, just so much *smaller* or *shorter* (*kürzer*) must the pause be between the phases if any kind of movement or any degree of fusion is to be seen." Read, therefore, *smaller* for *greater* in line 2 from the top of page 512 in the place cited above. The error in my statement of Cermak's fourth law was simply a translator's error, but important for anyone working on apparent movement—all of which goes to show that the student cannot dispense wholly with a knowledge of foreign languages and must be prepared to consult sources no matter how plentiful translations and commentators may be.

University of Kansas HARRY HELSON